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PERSISTENT PROBLEMS IN SYSTEMATIC PSYCHOLOGY I. A PHILOSOPHICAL HERITAGE

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A distinction between structure and function seems to be fundamental to all scientific theory.[1] These concepts represent two mutually exclusive and yet dependent points of view. While in the abstract the static and the dynamic constitute a logical parallelism or dualism that may be subsumed under a single word such as event-particle or content-process, in handling any specific problem the two viewpoints must be kept clearly in mind or else one is involved in paradoxes and infinite regresses. Moreover, failure to resolve any problem ultimately to this logical parallelism leads to artificial distinctions and to falsifications of data. That is, an attempt to envisage any scientific datum in terms either of structure or function, alone, makes impossible a consistent picture of nature and precludes the possibility of organizing scientific facts.

It is generally held that description ultimately refers to structure or content—the given—and that interpretation refers to function—the non-given, value, use, et cetera. Further, one is led to suppose that description furnishes us with a picture of the 'real' and is in itself an adequate scientific analysis of nature, with a static universe the ideal; that interpretation is theory in contradistinction to description; that description only has to do with the scientific datum or

[1] Wheeler, R. H., 'The Static and the Dynamic in the Logic of Science,' *Monist*, 1923, 33, 556–567.

the existential; and that, it is possible and expedient to separate the existential from the interpreted on methodological grounds. Accordingly, the existential is separated on logical grounds from a consideration of the datum in use and the latter approach to the scientific problem is regarded as technological. That these distinctions and assumptions are unsafe guides when followed out literally is one of the burdens of this paper and others that are to follow. More specifically, however, we shall deal for the present with a narrower issue, namely, that of the structure-function problem as it has been handed down to psychology from philosophy; and it will be our purpose to show that much which passes for sound psychological theory is a misconception of a neutral and logical problem.

A confusion with respect to this logical issue has resulted in the failure of one movement after another. Associationism, apperceptionism, the act and self psychologies, and biological functionalism are outstanding examples. No better proof of this fact is needed than Titchener's masterful lectures on the higher thought processes [1] and his recent papers on the psychology of act.[2]

Philosophical and epistemological problems have determined our definitions of psychology and our assertions as to what constitutes the psychological datum; and in a last analysis, these issues are responsible for the present and unwarranted cleavage between introspective and behavioristic psychology, a cleavage, which, we are glad to see, is slowly disappearing.[3] The differences of opinion represented by the introspectionist and the behaviorist are unquestionably based upon erroneous assumptions and historical prejudices. Nothing would be more tragic for psychology than to proceed on the theory that these differences in viewpoint are unsolvable because they represent temperamental differences in psycho-

[1] New York, Macmillan, 1909.

[2] *Amer. J. Psychol.*, 1921, 32, 519–542; 1922, 33, 43–83.

[3] Lashley, K. S., 'The Behavioristic Interpretation of Consciousness,' PSYCHOL. REV., 1923, 30, 237–272; 329–353.

Hunter, W. S., 'The Problem of Consciousness,' PSYCHOL. REV., 1924, 31, 1–31; 'The Symbolic Process.' PSYCHOL. REV., 1924, 31, 478–497.

Bentley, Madison, 'The Field of Psychology.' New York, Appleton, 1924.

logists,[1] or, on the other hand, that there are two distinct psychological sciences.[2] If temperament is the criterion by means of which to differentiate one science from another, certainly neither of those sciences can be trusted. On the contrary, the difficulty seems clearly to lie elsewhere and presumably in the problems which Titchener and Boring [3] have so forcibly brought to our attention.

Were it not for the fact that conventional psychology is lodged on the same rock which it has attempted to avoid, there would be ample ground for believing that function had been ruled out of court [4] and that the behavioristic point of view had legitimately been called a biological one. Our case will rest upon a discussion of parallelism and the double-aspect theory, the distinction between process and meaning, the stimulus-error and introspection.[5] The latter problem will bring us in touch with recent charges made against the Külpean and Bairdian tradition.[6] In other words, persistent difficulties in system-making are preventing a normal amalgamation of introspective and behavioristic psychology, and first and foremost among these problems is the structure-function antithesis. Today it is being largely misused or neglected. Its misuse may be traced into the history of philosophy and its neglect to a revolt against our philosophical heritage.

The assertion that primitive man was a dualist [7] may be seriously questioned. This statement seems to have been

[1] Pillsbury, W. B., 'Suggestions for a Compromise of Existing Controversies in Psychology,' PSYCHOL. REV., 1922, **29**, 250–266.

[2] Fernberger, S. W., 'Behaviorism versus Introspective Psychology,' PSYCHOL. REV., 1922, **29**, 409–413.

[3] 'The Stimulus-Error,' *Amer. J. Psychol.*, 1921, **32**, 449–471; 'Attribute and Sensation,' *Amer. J. Psychol.*, 1924, **35**, 301–304. Titchener, *op. cit.*

[4] Cf. Titchener, *op. cit.*, also, *New Inter. Year Book*. 1922, p. 595 *f.*

[5] We hesitate to share the belief that it makes no difference whether, as psychologists, we are dualists or monists. Philosophical and epistemological dilemmas are responsible for the fact that, today, we have psychologies and not a psychology. Not only does one's philosophical viewpoint influence selection and interpretation of facts, but it may also blind him to the most important facts of all.

[6] Boring, E. G., *J. Phil.*, 1924, **21**, p. 95; Pratt, C. C., *J. Phil.*, 1924, **21**, pp. 225–231.

[7] Klemm, O., 'History of Psychology,' New York, 1914, p. 14.

based upon the fact that, to him, the human being was a body and a soul. The soul was life and persisted after death. It is obviously a mistake to give the name, dualism, to this primitive view of things. The concept of soul was derived from concepts of external objects, even as late in human thinking as the time of the early Greeks, who described the soul in terms of water, fire, and air. No doubt primitive man developed an idea of force, based unknowingly upon his kinesthetic sensations, but that this is a spiritualistic concept is a notion which few of us would venture to entertain.

Dualism may be traced to three sources at least: vitalism and the concept of immortality, the problem of the static versus the dynamic, and the problem of the validity of knowledge. Originally, Greek thought was essentially materialistic, but very shortly took on a dualistic tinge.[1] For Heraclitus, mind was a stage in the evolution of fire. For Democritus, the soul was made of fine, smooth atoms, pervading the body, a soul-atom lying between two other atoms and producing bodily movements. Contemporary with this type of view there developed the extremely important notion that somehow things exist—including soul-phenomena—not as *sui generis* characters but as harmonies, proportions or combinations of units which in themselves partook of different properties from the combination. (Dicaearchus of Messene, Strato.) Materialism and atomism, however, failed to satisfy. They were obviously too narrow. Empedocles conceded that the soul was constituted of elements combined in characteristic proportions, but this was not the whole story. The soul must be something more than proportion—what it is, and not something else. And so, Empedocles, otherwise a materialist, paved the way for a dualistic conception by making the soul an entity which transcended the effects which we see—an entity which was capable of migrating from one individual to another. This was a dualism motivated by religious and mystical faith.

Another situation developed which was to make itself felt. The Pythagoreans found themselves struggling with the

[1] The material forming the basis of the next few pages has been taken from standard histories of philosophy.

problem of the limited versus the unlimited. Here dualism began to take definite form, not as an antithesis between a mind-substance and a material substance, but between the static and the dynamic. Then Anaxagoras conceived motion or change as a principle of order and intelligence. Motion was *nous*. The *function-member of the dichotomy is already taking on a mentalistic connotation.* But the concept seems to have been only a last expedient following a fruitless attempt to explain the universe on materialistic and mechanistic principles.

However, with the impetus now given to the idea that the soul is a real entity, it can easily be understood how such an influence as the cult of Dionysus, with its indulgence in abstracted processes of consciousness furthered the concept. The observer felt that he was transformed from one world to another. Mysticism again came to the aid of logical inquiry and *distorted the structure-function antithesis still further.*

The scepticism of the Sophists resulted in sharpening the issue between monism and dualism by forcing philosophy to examine the thinking process. To find validity in knowledge became the primary aim, and the problem of mind was now epistemological as well as metaphysical. Immediately the soul became almost an ego, an agent who knows. Accordingly, by the time we reach Plato we have the notion of a world-soul of mentalistic connotation and a failure on the part of philosophy to establish a criterion for truth. Faced primarily with an epistemological problem, but forced to seek support in a metaphysics for his theory of knowledge, Plato elevated dualism to another level and gave it a new twist, that of a dualism between appearance and reality—the phenomenal and the actual. It was also a dualism of values. Hand in hand with this developing dualism there slowly crystallized the concept of subject and object. The one problem depended upon the other and both were based upon the logical distinction between two mutually exclusive but dependent realms. In monistic systems the subject-object relation was either ignored, treated abstractly as a cause-effect relation, or merely as an implication.

That the dualistic position was satisfactory received frequent denials. For Aristotle, the soul was to the body what form is to matter. It was the entelechy of the living body—not a substance but a formative principle. The knower and the known were one. Saint Augustine adhered to monistic more closely than to dualistic principles. Descriptively the soul was identified with the life of the body. In man there was a combination of body and soul which resulted in a third substance and it was this third substance which we observed. On the other hand, the status of dualism was rendered somewhat more explicit by an emphasis on the self as a knower, who distinguished itself from object. Introspection was the *modus operandi* of self-knowledge. Thomas Acquinas adhered to the Aristotelian doctrine of matter and form, but here the problem became complicated by an inquiry into the nature of the universal and the particular. In his distinction between subsistent and inherent forms we have not so much a dualism of substances, a dichotomy savoring of the Platonic distinction between appearance and reality. In this dualism of the phenomenal and the actual we find the same logical problem as in the dualism of structure and function, content and act.

It remained for Descartes to crystallize the dualistic position. His motive was not unlike Plato's, for he was faced with the same problems, now accentuated, on the one hand, by a newborn faith in science and the mechanistic principle, and, on the other, by a long period of spiritualistic philosophy in which a vigorous attempt had been made to rationalize theology. The result was what one might expect—a sharp differentiation between mind and matter, subject and object, with interaction as a means of relating the two realms. But even in this extreme dualism of substances there is evidence that the real antithesis lies in structure and function. Matter, for Descartes, was passive. Mind was active. *The notion of substance is not therefore the logical kernel of the problem.* Out of the genuine problem of structure and function there had now developed a supposedly genuine problem, a dualism of substances, in which the function-side of the antithesis had

been given an additional content of its own. Matter, originally the content, was forced to seek another function, namely, that of physical energy, and mind was forced to seek another content, that of spiritual substance. *Out of the original dualism, two dualisms had been forged by theology and ethics.* Thus we see that psychology, which by rights should straddle but one dualism, has since the time of Descartes been straddling *two dualisms*. It was destined to shape itself according to the fashion in which metaphysics attempted to resolve these two dualisms into one again. But the mischief had been wrought. In its very inception as a distinct field of endeavor, psychology was removed from the realm of nature by two sweeping deductions. First, the subject-matter was consciousness—the soul—and second, the two realms, soul and nature, were segregated by supposedly incompatible attributes. This situation was not radically changed by subsequent alterations of the dualistic notion. It was not even changed by the later idealistic and realistic movements.

In Spinoza, a dualism of substances gave way to a dualism of attributes, a logical distinction by means of which the concept of mechanism was brought into relation with the concept of mind. We return part way to the original problem of structure and function. Extension and determination belonged to a category of material structures; mental life belonged to a category of functions. Logically, there has been a return to the Pythagoreans, but the real situation was camouflaged by a misconceived phenomenalism.

As a consequence of Hume's skepticism and Kant's denial of metaphysics, philosophy made frantic efforts to justify itself. The unknown must be defined or the metaphysical problem abandoned. Accordingly, we find in the development of idealism an attempt to offer such a definition in which the absolute principle is the ego (Fichte), an absolute mind (Schelling), idea (Hegel), will (Schopenhauer), and the unconscious (Hartmann). Since then realism, intuitionism, pragmatism, and objective idealism have had their swing.

Thus psychology developed in a dualistic and idealistic setting. English philosopher-psychologists had paved the

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way for a structural psychology under a dualistic influence (Locke, Hamilton, and Spencer). On the continent, Fechner and Wundt were idealists in theory, and dualists of a kind in practice. Wundt's doctrine of parallelism, which was intended to be merely a statement of an empirical situation, turned out to be only a circumlocution, for it had the same effect on method, on our definition of the datum and of the science, as if it had been literally an out-and-out dualistic, metaphysical conception. It was relatively a simple matter to emphasize the inductive and experimental method, but a more difficult problem to evade dualistically tinged philosophical preconceptions in the envisagement of psychological problems. The necessity for an exact delineation of the psychological field under these conditions made a definition of the science practically necessary but exceedingly hazardous. With either a dualistic or an idealistic prejudice or both, psychology was bound to evolve as a science of consciousness. Moreover, with an epistemological problem in the background as well, the definition of introspection was bound either to savor as much of philosophy as of science or else to take on an arbitrary and fictitious meaning. Not only the definition of the field but the conception of the method was largely a by-product of philosophy.

A large number of present-day psychologists apparently cling to a dualistic position so altered as to avoid the absurdities of interactionism, but not sufficiently changed to escape other logical consequences of a dualism of substances. The concepts of attribute and aspect perform the same logical rôle in thinking as the concept of substance. They are a recourse to which one is driven by the necessities of description and analysis. They serve the purpose of content. Whether we construe that content as aspect or as substance, the dichotomy is logically rigid and the consequencies are artificial. Mental and physical processes are as far removed from one another as aspects of an unknown as were Descartes' mind and matter. The problem of relationship has not been solved. The issue has been neglected, but the same old problem admitted. In this connection the analogy of the coin with its

two sides, or the arc with its concave and convex aspects is misleading. We can relate one surface of the coin with the other and one side of the curve with the other because the middle term is known. But to assume that in observing a mental process, on the one hand, and a physiological process, on the other, we are obtaining two views of the same process attempts to relate two knowns by means of an unknown, which latter, by implication, is neither one aspect or the other. Not so with the legitimate scientific theory which assumes that the unknown partakes of the same order of content as the known, hence the problem of relationship can be envisaged.

The reason for this dilemma is to be found in the metaphysical nature of the double-aspect theory. In the Cartesian dualism logic demanded that a relationship be hypostasized between mind and body. Interactionism resulted, followed by occasionalism. To posit an unknown of which mental and physical processes are different aspects is merely a substitute for the same mysterious third factor. It is because the double-aspect theory concedes this third factor without recourse to the structure-function problem that it is logically as much a metaphysical view as its antecedent. What is the difference? In the one case the third factor is an 'unknown' and in the other it is God. And in another type of dualistic theory—the old parallelism—it was 'nothing' positively envisaged! We raise the point therefore that the double-aspect theory is no better than its parent.

Further, both theories—double-aspect and psychophysical parallelism—lead to the logical difficulty of an assumption which precludes the possibility of scientific demonstration. A mental process is supposed to be one aspect and the physiological process another aspect of the same 'thing.' Unless we wish to lay ourselves open to the difficulties of positing a mentalistic subconscious we must assume that a mental process does not exist when not subject to observation. This would mean that there is a certain physiological process which we must assume does not exist unless its mental aspect is observable! In other words, like the theory of the subconscious mind, the double-aspect position logically precludes

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the possibility of verification. Such a theory has no place in the realm of science. Parallelism leads to the same absurdity. Hypostasize a one-to-one relation between a mental process and a physical process and the assumption is unavoidable that when a certain mental process does not exist the physical process also does not exist; the proof of which the situation prohibits.

We should not expect an unsound theory to produce a wholesome influence, and since the two theories in question are so general in nature their unsound effects are extremely far-reaching. First among these is an artificial structuralism. Science everywhere deals with function as well as with structure, and description has to do with successive stages in the transference of energy. Structuralism alone has no claim to the name of science but is a method in science, and until supplemented by functional concepts it is logically barren. Parallelism and the double-aspect theory limit psychological procedure to a description of consciousness and a classification of mental processes. These processes can be related to nothing—not even themselves—except by way of statistical correlation. The latter is purely empirical; it adds nothing to the situation; it knows no logic; one finds what he finds and that is all. We are prohibited logically from envisaging a common denominator by means of which to relate one datum with another unless we adopt the principle of a spiritual entity. Otherwise, there is no way of conceiving a psychic continuum.

The functional problem is only partly handled by having recourse to concepts like fluctuation, assimilation, disintegration and the like. The assumption of an active principle is unavoidable and so we have had our concepts of energy in physics, striving, apperception, et cetera, in psychology. The logical value of such concepts lies in their abstractness and universality. They fit the demand for a common denominator and save science from being a statistical procedure devoid of logic. Lacking such an active principle, interpretative expedients such as *Aufgabe*, determining tendency, perseveration, and incorporation are empty constructs. To put the burden upon physiology and at the same time to adhere

systematically to a dualistic position does not provide mental phenomena with a common denominator.

As it is, physiology is helpless in giving psychology any genuine interpretative aid which can be understood, and psychology is helpless in an endeavor comprehensively to solve the problems of human nature. Social psychology seeks in vain for help from psychophysics. As a consequence also, genuinely psychological problems have long remained unsolved and the more recent advances in science upon which the psychologist has been forced to depend, have come from extra-introspective studies—physiology and the study of animal behavior, researches on the learning process and the like. Witness the contributions of Pavlov, Crile, Sherrington, Cannon, Carlson and their co-workers, the mental test movement, and psychoanalysis, the results from all of which have filled the gaps in our newer text-books. An incorporation of such results into the psychologist's classification of data has been done with a conscious and legitimate disregard for metaphysics and with an implicit recognition of the fact that such data are as much a part of psychology as the results of the chemical analysis of digestion are a part of physiology.

In short, psychology is no more a description of consciousness than biology is a description of muscles and bones. Such descriptions constitute only a part of each discipline, a part which supplies differentiæ, to be sure, but more than differentiæ are needed. We are in need of a few general interpretative principles. As Titchener has pointed out,[1] the behavioristic movement is a reaction against a functional psychology that was teleological, on the one hand, and epistemological, on the other, but it is also a reaction against artificiality and the metaphysical implications which have ever stood in the way of making the facts of psychology scientifically intelligible and capable of being related to other facts in a consistent, logical way. It is quite as much opposed systematically to Wundt as to Brentano, to Titchener as to Angell. While the immediate excuse for such a reaction was a search for supposedly more reliable methods, methodology hinges

[1] *Op. cit.*

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upon the definition of the science and the scope of the field in which the method is applied.

In attempts to find criteria for the psychic, we have further evidence of the arbitrary and unwarranted dichotomy between mind and body. Attempts to make the criterion one of non-extendedness, or of immanent objectivity, or of experience dependent upon an experiencer have summarily failed to satisfy. Even the criterion of awareness has failed to stem tide after tide of belief in the subconscious. Had the relationship between mind and body been definitely envisaged as a structure-function problem these failures, no doubt, would have been avoided; and if the relationship between process and meaning—a similar problem in a last analysis—had been envisaged in the same way, we probably would not have had the artificial and misleading distinction between sensation and sensation-of, idea and idea-of, which allocates mental processes, analyzed, to psychology, and synthesized, to logic or epistemology.[1]

It has been the thesis of this paper that attempts to solve the mind-body problem on other grounds than the relationship of structure to function have resulted in an arbitrary and unscientific definition of psychology, forcing upon it an artificial structuralism and a host of semi-functional concepts which assume the position of science while precluding the logical notions adequate to a definition. Its difficulties can hardly be explained on the ground that venturing into interpretation is either premature or impractical. We cannot go to the extreme of asserting that science is 'pure description,' nor can we accept too literally the view that explanation is mere description. Description is in part interpretation. In methodology interpretation plays quite as important a part as experimentally controlled observation. Here analysis and interpretation go hand in hand for without the former the latter is speculation, and without the latter the former is disorganized effort in which experimental work proceeds unguided and method suffers for the want of ideas. We must always have therefore interpretation. We submit that in

[1] Cf. Titchener, *op. cit.*, 1912, 23, 427–448; 485–508; 1915, 26, 258–267.

psychology interpretation is still suffering—has already suffered—from philosophical prejudices. We believe that basic notions in psychology will never be generally acceptable until these prejudices and the mischief they have wrought are forgotten and psychology redefined accordingly. A psychology defined as the science of behavior—a behavior in which mental processes play a genuine rôle—gives us the desired freedom from metaphysical and epistemological difficulties when the background is a logical, not a metaphysical monism, and the method is a combination of the 'objective' and introspective procedures. These two procedures are not to be distinguished by ultimate differences in the nature of the subject-matter but by the relative accessibility of the observer to it; and the feature which identifies this background as logical and neutral is the purely logical, structure-function antithesis.[1]

[1] A fuller statement of the mind-body relation as a structure-function relation is to be found in Wheeler, R. H., 'Outline of a System of Psychology,' PSYCHOL. REV., 1923, 30, 151–163. Cf. Dewey, *Phil. Rev.*, 6, 44. *Mind*, 12, 384. Bawden, *Phil. Rev.*, 1902, 11, 474–484.

AN EVALUATION OF CURRENT SENSATIONISM

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I. The Structural View of Sensation

The term sensation has had many meanings and many interpretations in the history of psychological systems. Today, once again, this concept is seriously challenged. In this paper an attempt will be made to evaluate some tendencies in current sensationism and to present tentatively what the writer considers to be a constructive position upon those phenomena now considered as sensation.

In a review of the doctrine of sensation as it is now propounded, the teaching of the so-called structural school is of first importance, for this group of laboratory workers has possibly given more time to a consideration of this term than has any other body of psychological thinkers. In this school at present, no single name stands out more prominently as regards a serious study of sensation than that of Professor E. B. Titchener of Cornell University. In this paper therefore we propose first to consider the views of this experimentalist upon sensation. The task of presenting the point of view of Professor Titchener upon any theoretical subject is not an easy one. The interest of this writer has been largely centered in experimentation rather than in the logical development of any hardened conceptual system. Indeed, he has recently proclaimed that,

> Personally I do not greatly care about theoretical views; they are nothing more than an individual's blundering efforts to bracket together and make manageable some large unruly body of observed facts.[1]

This attitude of at least comparative disregard for logically oriented systematic positions has caused in the past great trouble for students who have essayed the task of evaluating Professor Titchener's exact views upon specific psychological

[1] Titchener, E. B., 'Wilhelm Wundt,' *Amer. J. Psychol.*, 1921, 32, 176.

problems. Moreover, in reading the polemic papers of this writer one notices that, according to their author, many of the objections to his standpoints have an origin in the fact that the critic has not correctly understood the material upon which he adversely comments. In order to obviate this difficulty the contentions of Professor Titchener will be presented in this paper, in so far as it is expedient, in quotation rather than in paraphrase.

Turning, then, to Professor Titchener's work we find the following statement upon sensation in his 'Experimental Psychology':

> If we take some such section of consciousness . . . and analyse it, we find that it reduces to a number of quite simple processes, all of the same general kind or class. These are called *sensations*. The sensation, then, is the structural unit or structural element of these consciousnesses,—just as the cell (so the anatomists and physiologists tell us) is the structural element of our bodily tissues. If we wish to understand the make-up of mind, we must know all about these sensations.[1]

And, again, in the 'Instructors' Qualitative Manual' we read,

> The sensation *is* strictly subject-matter for psychology. . . . It is an ultimate process of structural analysis. . . . This sensation which is a structural unit of the adult mind, is not the genetic unit of mind at large. Mind has not grown by aggregation of sensations. . . . At the time when the heart begins to pulsate, there are no muscle fibrillæ in the myocardium; we have the sight of purely protoplasmic, undifferentiated cells making strong rhythmical contractions. If, now, the structural elements of the primitive mind are 'sensations,' they are sensations only in the sense that these primitive heart-cells are 'muscle' cells. . . . Nor is this sensation a functional unit of mind. It is not the tiniest bit of mind that can give us knowledge, not the simplest form of knowing. Knowing, i.e. does not come, any more than does mind, from the addition of sensation to sensation. The sensation 'blue' does not tell us of a blue object, does not even tell us that it comes from the eye. It simply presents itself as a mental irreducible; and we have to examine it for its own sake. . . . There is no gap, and therefore no need of a bridge, between sense psychology and the psychology beyond sense.[2]

In a similar manner we are told in the Lectures on the 'Experimental Psychology of the Thought Processes,' that,

> A great deal of confusion would be avoided if psychologists at large recognized the fact that the sensation of experimental psychology is a simple, meaningless (or, rather, non-meaningful) process, definable only by an enumeration of its attributes. . . . It is, for instance, axiomatic for the experimentalist that a sensation

[1] *Idem*, 'Experimental Psychology,' Vol. I, Part I, 1901, pp. 1 *f.*

[2] *Idem*, 'Experimental Psychology,' Vol. I, Part II, 1901, pp. 3 *f.* Paragraphing changed.

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> cannot function alone; at least two sensations must come together if there is to be a meaning; the single element can do nothing more than go on; so far as cognition or function is concerned, *sentire semper idem, et non sentire, ad idem recidunt.*[1]

Likewise, in the 'Text-Book' we read,

> The three classes of elementary processes are known as sensations, images, and affections. Sensations are, of course, the characteristic elements of perceptions, of the sights and sounds and similar experiences due to our present surroundings. . . .
>
> . . . . . .
>
> A sensation as the term is used in this book may be defined as an elementary mental process which is constituted of at least four attributes,—quality, intensity, clearness, and duration. There are sensations for which the list of attributes is longer; but these four are essential. . . .[2]

It is important to notice that the term 'mental-process' is used here as descriptive of sensation even in this so-called structural system of psychology.[3] The exact meaning that Professor Titchener puts upon this phrase, 'mental-process,' is made clear in the following quotation,

> Mind thus becomes the sum-total of human experience considered as dependent upon a nervous system. And since human experience is always process, occurrence, and the dependent aspect of human experience is its mental aspect, we may say more shortly, that mind is the sum-total of mental processes.[4]

In answer to the problem of why the sensation should be considered as the element we are told in an article on 'Sensation and System' that,

> . . . the sensation of classification is the logical resultant of many observations. Its qualitative attributes are (in the typical case) points selected by definite procedure from a continuum; its intensive attributes are themselves continua, reduced by the same procedure to a series of points. Hence the conjunction of the qualitative attributes with any points whatsoever upon the correlated intensive scales constitutes, for classification, 'a' sensation; the bracketing together of the qualitative attributes with the complete intensive scale constitutes 'the' sensation. It would be a great simplification of psychology if a sensation, *toto, teres, atque rotunda*, would stand before us under a single comprehensive determination and allow us adequately to observe it as a whole. But that, if it ever happens, happens only after we have made many separate observations of its distinguishable aspects.[5]

Furthermore, the admission is made that the choice of the sensation and not the attribute as the descriptive ultimate, is

[1] *Idem*, 'Lectures on the Experimental Psychology of the Thought-Processes,' 1909, Note 28, pp. 214 *f.*

[2] *Idem*, 'A Text-Book on Psychology,' 1910, p. 48 and 53.

[3] 'Process' as used in this system is in no sense to be confused with 'function.' Cf. quotation given above from the 'Instructors' Qualitative Manual.'

[4] *Ibid.*, p. 16.

[5] *Idem*, 'Sensation and System,' *Amer. J. Psychol*, 1915, **26**, 261.

logically rather than psychologically determined, for the same writer continues,

> Observation is in fact conditioned upon two things, peripheral reception and central disposition,—conditioned, as I have put it, upon excitation and determination. We group certain attributes together, in our classification, because, having observed one attribute under a certain determination, we thereafter invariably light upon certain other attributes when, with shift of determination but with excitation unchanged, we undertake to observe them. . . . We regard the attributes as constitutive, again, because the grouping or clustering is thus invariable. . . . In logical terms, the reduction to zero of any member of the attributive group destroys the whole process of sensation. And lastly, we give excitation the logical preference over determination, and take the 'sensation' rather than the 'attribute' as our mental element, because change of determination is limited in its effect to the unravelling of the groups or clusters already referred to . . . it shows us nothing more than the inseparable aspects of some simplest item of our subject-matter; whereas shift of excitation brings in new matter. We cannot pass, by mere shift of determination, from a hue to a sour; but by mere shift of excitation we may pass from hue to any other sensory quality that there may be.[1]

Sensation, moreover, as conceived by Professor Titchener must not be judged upon its adequacy to make up complexes; it is an analytic and not a synthetic conception. To the contention that there must be error in a method which can analyze into elements but not synthesize into wholes, this same writer answers, in a way which may satisfy some thinkers, that,

> . . . a description, as compared with an intimation of meaning, is always clumsy and long-winded; but, secondly, that psychology—as I regard it "is under no obligation to reconstitute, at the end of an investigation, the [continuum of] psychical experience; it is enough to have shown that its contributions to the analysis of that experience are valid." In other words, as I have said elsewhere, a descriptive science may not justly be asked to give more than, by its nature as descriptive science, it can properly furnish.[2]

The error of insisting upon the synthetic possibilities of the structuralist's logical construct called a 'sensation' lies, we are told, at least in some measure in the tendency upon the part of psychologists to confuse the teachings of the structural group with those of some other psychological school. This position is emphasized in the following quotation from Professor Titchener:

> It would indeed be surprising, were it not so common, to note how the critic of current sensationalism persists in ascribing to sensation, almost as if by instinct,

[1] *Ibid.*, pp. 261 *f.*
[2] *Ibid.*, p. 266.

> the characters which the associationist school attributed to idea. In the typical associationist system the idea was, first and foremost, a meaning; as a meaning it was stable and permanent; and its association to other and similar ideas gave rise to all kinds of mental formation. We may insist, now, that meaning has been pared from sensation,—we may insist that meaning, from the psychological point of view, is in general something detachable, something that accrues, something variable and incidental; yet we cannot drive that point home; our critics still write as if sensation involved awareness.[1]

The elements tentatively [2] assumed by this school are, of course, besides sensation, those of image and affection.[3] It is held that these three processes may expediently be separated. The difficulties of this view are considered below.

A point has now been reached at which a summary of the views of Professor Titchener upon the nature of sensation may be offered. Before attempting such a digest, however, it is only fair to suggest that, not only may the conclusions which we draw upon this subject be inadequate; but, also, that Dr. Titchener himself may have modified his views in certain respects since he wrote the sources to which we have made reference. Indeed, in 1922, Dr. Titchener has written,

> There can be no possible question that sensation—however it is to be defined; and we need not, for a long time to come, inquire too curiously about its definition—has been from the beginning a source of real difficulty to the act-systems.[4]

In the present paper, we cannot postpone for this 'long time' an effort to determine something of the *current* teachings of the structural school upon sensation, where, we may add parenthetically, as well as in the act systems, it seems to be a 'source of real difficulty.'

We therefore present the following nine factors as an indication of some of the important contentions of the structural school upon sensation: (1) The sensation is a logical construction and it is not, at least primarily, observable. (2) The sensation is simple. (3) The sensation is a process. (4) The sensation is a structural element of consciousness. (5) The sensation is the sum of its attributes and it is not something over and above this. (6) The sensation if it is to be a sensa-

[1] *Ibid.*, p. 265.

[2] Cf. *idem*, 'A Text-Book of Psychology,' pp. 46 *ff.*

[3] *Ibid.*, p. 49.

[4] *Idem*, 'Functional Psychology and the Psychology of Act: II.,' *Amer. J. Psychol.*, 1922, 33, p. 74.

tion must have the following four essential attributes: quality, intensity, clearness, and duration; it may have others, but it must have these. (7) The sensation is non-meaningful, and it has nothing to do with awareness. (8) The sensation is an analytic, not a synthetic conception; and it is not genetic. (9) Sensation may be distinguished from image and affection.

II. Criticism of the Structural View of Sensation

Many criticisms have been urged against the structural view of sensation which has been sketched above. These criticisms may be roughly dichotomized. The first group consists of those who criticize from within the general structural viewpoint; the second, of those who criticize from without the system. The objections, in other words, of puritans and separatists. Here, we shall consider the former group of criticisms first; turning only secondarily to those objections which are so absolute as to mark a distinct break from the whole structural systematic position.

The first objection which may be offered is that it is difficult to see how a systematic construct that is proclaimed as simple, ultimate, and elemental can have separate attributes; or, more strictly speaking, be the sum of such attributes. This objection to the definition of sensation as here presented has been urged in the past. Therefore, before considering in detail its cogency, we may well note how Professor Titchener tries to justify his position against some of these very attacks to which we shall make reference. He says,

> We have taken it for granted that the mental elements may be arranged in groups or classes. It may be objected that, since they are elements,—since they resist analysis, and cannot be reduced to anything simpler than themselves,—we can have no means of grouping them. How is it possible to find degrees of likeness and degrees of difference between absolutely simple things?
>
> The psychologist arranges mental elements precisely as the chemist classifies his elementary substances. The chemical elements are divided, for instance, into metals and non-metals. The metals have a high power of reflecting light; they are opaque; they are good conductors of heat and electricity. . . . So they are set off, as a group, from the non-metals. . . . That is to say the chemical elements possess certain properties or attributes, by means of which they may be distinguished and arranged.
>
> It is just the same way with mental elements. . . .[1]

[1] *Idem*, 'A Text-Book of Psychology,' pp. 49*f*. Cf., also *idem*, 'Sensation and System,' *loc. cit.*, p. 262.

An inspection of the above defense shows that it rests entirely upon the argument from analogy and more than this, that the analogy is not, it seems, a particularly fortunate one.[1] In the first place, the situation of the so-called 'element' is far from secure today in the science of chemistry. It would be in no sense an exaggeration to say that the modern chemist tends more and more to the view that the nominal 'elements' are in no real sense the end point of analysis.[2] In the second place, an analogy between a structure-process such as the mental element is held to be by this school, and a gross, tangible, static mass of a 'chemically pure' element, large enough to have the physical properties enumerated, is almost certain to mislead.

It seems, moreover, that the weakness of this analogy is but an indication of the fragile nature of the whole case for the doctrine of sensory attributes. The objections to this view have been brought out by a number of writers and an introduction to the argument upon the topic may be well secured by a reference to this critical literature.

The whole problem of psychological atomism is considered by the late Professor Münsterberg.[3] He points out that analysis is an essential of the scientific method, and that the method of psychology is no exception to this general rule. 'The internal microscope of attention'[4] he picturesquely terms the only instrument that he believes can be used for the analysis of mind. He emphasizes the importance of sensation by saying that,

> The general trend of this modern analysis of will and emotion can be understood from one point: they are described as combinations of sensations, that is as combinations of such elements as we find in our ideas.[5]

These sensations, however, he points out, differ in regard to quality, intensity, and vividness. But, he asks, if the sensations thus differ are they to be thought of as ultimate ele-

[1] Cf. Rahn, C., 'Sensation in Contemporary Psychology,' PSYCHOL. MONOG., 1913, No. 67, p. 20.

[2] Cf. Loring, F. H., 'The Chemical Elements,' 1923, pp. 95 *ff.*

[3] Münsterberg, H., 'Psychological Atomism,' PSYCHOL. REV., 1900, 7, pp. 1–18.

[4] *Ibid.*, p. 1.

[5] *Ibid.*, p. 2.

ments? All agree that the 'idea' of the older psychology is not simple; is it not possible that the sensation is also complex? The physicist, he tells us, has espoused what may be called a radical atomism and has found the conception so developed a helpful one. He suggests therefore that psychology might also find a radical atomism of its own advantageous, if it can discover any really elemental ultimates, for he declares that, "Sensation is not, as is usually said, the simplest element which psychological analysis can find." [1] Indeed, he believes that all sensations may be reduced to 'psychical atoms.' These atoms differ from sensations in that they have no direct relation to the stimulus, and that each one is held to be different from each other atom; while, on the other hand, sensations are, in some measure at least, similar one to another.[2]

In a differently conceived paper upon this same general subject, by Miss E. B. Talbot, we find a defense of the attributive view. She says in regard to the problem of analysis that,

> We start with our complex fact, and seek first to reduce it to a simpler, more nearly elemental form. We explain it, primarily, by referring it to some simpler facts. . . . We have explained our fact by tracing it back to forms which themselves cannot be explained, but must simply be accepted.[3]

When faced, however, with the question of what the nature of this irreducibility may be, Miss Talbot maintains that it is to be defined in terms of the process of analysis that produces it. Therefore she is able to urge that the attribute is not an element of an element; for she holds that the analysis by which the sensation is analyzed out of the complex is quite another sort of analysis from that in which the attribute is isolated from the sensation. Thus she contends that,

> There is one point to be noticed with regard to the irreducibility of our ultimates. It is the fact that the work of analysis does not actually cease when they have been discovered. We have said that when we have resolved our facts into facts which are themselves irresolvable our process of analysis is finished. This is true; yet it would not be correct to say that there is no further occasion for analysis. There is need for a second process for the purpose of determining the properties of our elements. If we have agreed, for example, to recognize sensation as

[1] *Ibid.*, p. 8.
[2] *Ibid.*, pp. 15 *f.*
[3] Talbot, E. B., 'The Doctrine of Conscious Elements,' *Phil. Rev.*, 1895, 4, 154.

> ultimate, we may then proceed by means of analysis to discover its various attributes, such as intensity and quality.[1]

This defense offered by Miss Talbot seems to be very weak, for in no way are we informed in what manner the second process of analysis differs from the first. Indeed, on the contrary, the second process seems to be a mere continuation of the first analytic procedure; and this is particularly the case if we accept Professor Titchener's view [2] that the sensation may only be known after its separate attributes have been determined. Moreover, it seems that this attempt to differentiate products in terms of the analyses by which they are made known is a questionable procedure.

The arguments against the view espoused by Miss Talbot is well stated by Miss M. W. Calkins. This latter writer says,

> . . . either the sensation has attributes, but then it is complex, no element, and has lost its excuse for psychological being; or the sensation is an irreducible and unanalyzable element, but then its simplicity is absolute, not to be trifled with, and not to be explained away by reference to any second process of analysis into elements, which yet are not elements, but only 'attributes,' 'aspects' or something equally vague and meaningless.[3]

Professor Calkins' argument seems to be clear and successful in its full context, and, so far as the present writer can determine, it has never been satisfactorily answered. Professor M. F. Washburn,[4] Dr. Carl Rahn,[5] and Dr. E. B. Holt [6] all refer to the difficulty of the attributive position. Under the direction of Professor E. G. Boring this problem has recently, to a certain extent, been subjected to experimental study with interesting if somewhat equivocal results. The research in which this problem was attacked was conducted by Dr. Yokoyama in the Clark University laboratory. He made an effort to discover whether an observer who is maximally determined for one attribute can observe any other attributes.[7] In the experiment the observers, after fixating a

[1] *Ibid.*, p. 163.

[2] Titchener, E. B., 'Sensation and System,' *Amer. J. Psychol.*, 1915, **26**, 261.

[3] Calkins, M. W., 'Attributes of Sensation,' PSYCHOL. REV., 1899, **6**, 510.

[4] Washburn, M. F., 'Notes on Duration as an Attribute of Sensations,' PSYCHOL. REV., 1903, **10**, pp. 420*f.*

[5] Rahn, C. *op. cit.*, pp. 23*f.*

[6] Holt, E. B., 'The Concept of Consciousness,' 1914, pp. 210*ff.*

[7] Boring, E. G., 'Attribute and Sensation' (A note upon the unpublished work of Dr. M. Yokoyama), *Amer. J. Psychol.*, 1924, **35**, 301.

point of light were presented in the Dodge tachistoscope, with two rectangles of light end to end. These figures could be varied in either brightness or length. In one series the observers were strongly instructed to observe intensity, but about once in 50 trials they were asked, "How about extent?" Similarly, in a long series of judgments upon extent, intensity was called for at infrequent intervals. The results disclosed among others were that,

> If the capacity to make such a relative judgment is taken as the test of the presence or absence of an attribute, then it would be necessary to conclude that under a strong determination one attribute might be present and the others missing. Thus the complete selectivity of determination would be established.[1]

Dr. Boring, however, upon the evidence of introspections not available to the present writer, concludes that, due to the intermediation of a subsequent 'surrogate' for the perception, the observers were not able to reduce the perception observationally to a single attribute. As Professor Titchener predicted in the quotation given above, this experiment seems to show that the sensation does not stand before us in every case under a single comprehensive determination. In other words, in the psychological laboratory, at least in some cases, we deal with attributes and talk logically of sensations.

The first difficulty with the structural view of sensation is thus the equivocal relationship of the so-called element and the attribute. The second objection, to which we now turn, is that the conception of sensation which is sponsored by this school is by no means clear in the separation which it sets up between itself and the other assumed conscious elements.

The difficulty of distinguishing in a purely subjective psychology between the effect of present and past stimulation has long been recognized. Indeed, when our logical presuppositions lead us to talk of consciousness without reference to the stimulus we soon come face to face with this problem. It is therefore not surprising to find one of the first significant efforts toward a solution of this question in the writings of Bishop Berkeley. This idealist suggested that "The ideas of

[1] *Ibid.*, p. 303. Cf., also, Titchener, E. B., in quotation above upon the relative rôles of excitation and determination as logically justifying the use of the sensation rather than the attribute as the mental element, and O. Külpe, *Versuche über Abstraktion, Bericht über den I. Kongress f. experimentelle Psychologie in Giessen*, 1904.

sense are more strong, lively, and distinct than those of the imagination." [1] Since 1710, it almost seems that little advance has been made over this characterization. Professor Titchener himself proclaims that,

> It is usually said that the image differs from the corresponding sensation in three respects: its qualities are relatively pale, faded, washed out, misty; and its intensity and duration are markedly less. . . . But the writer is not sure that the image does not, as a rule, evince a sort of textural difference from sensation; that it is not more filmy, more transparent, more vaporous. If this is the case, it is better to consider sensation and image as sub-classes of a particular type of mental element than to include them outright in a single class.[2]

We have seen above, however, that the reason for speaking of the sensation rather than the attribute reduced itself in the last analysis to the fact that "we give excitation the logical preference over determination." [3] But certainly the process by which an image is aroused is more comparable to 'determination' than to 'excitation'; for it is not the immediate result of peripheral stimulation. Therefore it would seem that the attribute in the case of the image, at least, rather than the complex should 'receive the logical preference.' [4]

In a similar way there is difficulty in discovering how the structural school can logically differentiate sensation from affection. We are told that sensation, image, and affection, "all three may with some show of probability be viewed as processes of the same ultimate type." [5] And yet in the same work, we are given two criteria for the separation of the sensation from affection.[6] Similarly, in the 'Lectures on the Elementary Psychology of Feeling and Attention' seven [7] criteria are considered in an effort to arrive at the true distin-

[1] Berkeley, G., 'Treatise Concerning the Principles of Human Knowledge,' 1710, Section 25.

[2] Titchener, E. B., 'A Text-Book on Psychology,' pp. 198 *f.*

[3] See quotation above from 'Sensation and System.'

[4] Professor Titchener's characterization of the image always seems somewhat apologetic. *Vide* above. Cf. also, Titchener, E. B., 'Systematic Psychology,' *Amer. J. Psychol.*, 1921, **32**, in which a criticism is offered of Professor Stumpf's views as expressed in his monograph, 'Empfindung und Vorstellung,' *Abh. d. kgl. preuss. Akad. d. Wiss., J.* 1917, *phil-hist. Klass.* No. 8.

[5] Titchener, E. B., 'A Text-Book of Psychology,' p. 48.

[6] *Ibid.*, pp. 231 *f.*

[7] Six, plus the note on p. 43.

guishing characteristics of the two constructs.[1] The opposition of pleasantness and unpleasantness and the fact that the affections lack the attribute of clearness are the most important of these criteria. We saw above that each sensation must have, by definition, the attribute of clearness, and so by fiat of the term-maker, we may say that all 'integrations' which lack clearness and have certain other characteristics are feelings. But such a distinction is non-psychological, artificial, and therefore it is unsatisfactory for a descriptive science and has been accepted by few psychologists.[2]

Thus it appears that there is a fundamental difficulty in the structuralists' conception of sensation not only because of the difficulty of reconciling the attributive view with the elemental view, but also that the elements themselves are to be separated only in the most equivocal manner.

The third objection which we raise grows out of the two objections which we have already considered. It is, namely, that experiment does not always demonstrate the value of the structuralist's sensation. If this is the case, there can be no reason for claiming that the sensation is more fundamental than any other integration of attributes; for we have seen that it is the contention of this school that the systematic generalization is merely a pragmatic device for ordering large bodies of experimental evidence.[3]

We have already seen in the study of Dr. Yokoyama reported by Professor Boring that in relative judgments, at least, one attribute may be present but the others missing.[4] Dr. F. L. Dimmick in a study of visual movement or the 'phi phenomenon' made in the Cornell laboratory comes to the conclusion that this appearance may be described as "a primary integration of visual quality (gray) with a duration which is characteristically brief." [5] Now, in this result of an

[1] *Idem*, 'Lectures on the Elementary Psychology of Feeling and Attention,' pp. 36–78.

[2] A recent writer who accepts the attributive view and yet still rejects the notion that the sensations are different from the feelings is Professor K. Dunlap. Cf. 'The Elements of Scientific Psychology,' 1922, pp. 312 *ff.*

[3] Cf. Quotation above from Titchener's 'Wilhelm Wundt,' *loc. cit.*

[4] Boring, E. G., *loc. cit.*

[5] Dimmick, F. L., 'An Experimental Study of Visual Movement and the Phi Phenomenon,' *Amer. J. Psychol*, 1920, 31, 332.

experiment conducted in Professor Titchener's laboratory we are dealing with attributes in a so-called 'primary integration' which is no sensation. It has but two of the four 'required' attributes. Certainly, likewise, it is no feeling or image, and so it seems that we must face the experimental facts and admit that the attribute is more valuable, at least in this case, than the sensation as a descriptive 'ultimate.'

Similarly, in 1920 Professor Titchener published a brief note in which a model for the demonstration of sensory qualities on the skin is described. This model 'marks the direction of recent work.'[1] In this demonstration we find, unlike the earlier one for color, various modes arranged in continua. Again, in a note on 'Attensity' this same writer says:

> In systematic lectures of the last half-dozen years I have started out not from the 'mental elements,' but from the ultimate 'dimensions' of psychological subject-matter: quality, intensity, protensity, extensity, and attensity.[2]

Thus it seems that at least in some of the experimental work of the structural school itself the attribute and not the so-called 'mental element' is coming to be the end term of analysis. We may summarize therefore the three difficulties which we have found in the structural view of sensation under the following heads:

1. It is difficult to see how a logical 'simple element' may be composed of separate attributes.

2. It is difficult to see how, according to the criteria offered, sensation can be separated from affection and image, but such separation is necessary if the attribute is not to be considered the real ultimate.

3. It appears that the conception of attributes and the various integrations of attributes is at least sometimes a more valuable experimental device than the notion of mental element.

III. The Treatment of Sensation by Those Who Reject the Method of the Structural School

From the point of view of what is termed 'radical behaviorism' the conception of sensation is of no value in the

[1] Titchener, E. B., 'Models for the Demonstration of Sensory Qualities,' *Amer. J. Psychol.*, 1920, 31, pp. 212 *f.*

[2] Titchener, E. B., 'The Term 'Attensity,'' *Amer. J. Psychol.*, 1924, 35, 156.

scientific study of psychology. Dr. John B. Watson, for example, as a representative of this position tells us in introducing his now famous 'Psychology: From the Standpoint of a Behaviorist' that,

> . . . the reader will find no discussion of consciousness and no reference to such terms as sensation, perception, attention, will, image and the like. These terms are in good repute, but I have found that I can get along without them both in carrying out investigations and in presenting psychology as a system to my students.[1]

Other members of this behavioristic school also reject sensation *in toto*, although this is not the case in the writings of all the members of this school.[2] Closely associated with the rejection of sensation by the behavioristic psychologists is the similar renunciation of this and all other subjective terms by the 'objective' physiologists. This tendency was made explicit by Drs. Beer, Bethe, and v. Uexküll.[3] The use of any words characterizing purely mental life were also consistently avoided by the late Professor Jacques Loeb.[4] Dr. W. von Bechterew has suggested a program for psychology which is not concerned with mental terms.[5] In a similar way, Dr. I. P. Pawlow says of his researches upon the cerebral hemispheres and his studies of the conditioned reflexes that, "I never use any psychological conceptions or terms." [6]

This paper is not the place to evaluate the various objections to the view that psychology can afford to disregard the study of 'consciousness.' [7] If radical behaviorism is ac-

[1] Watson, J. B., 'Psychology: From the Standpoint of a Behaviorist,' 1919, p. viii. Cf. also, *Idem*, 'Psychology as the Behaviorist Views it,' PSYCHOL. REV., 1913, 20, pp. 163 *f.*, *Idem*, 'Is Thinking Merely the Action of Language Mechanisms?' *Brit. J. Psychol.*, 1920, 11, pp. 89 *ff.* and, *Idem*, 'The Place of Kinaesthetic, Visceral, and Laryngeal Organization in Thinking,' PSYCHOL. REV. 1924, 31, pp. 339 *f.*

[2] Cf. Roback, A. A., 'Behaviorism and Psychology,' 1923, especially Chap. II.

[3] Beer, T., Bethe, A., Uexküll, J. v., '*Vorschläge zu einer Objectivierenden Nomenklatur in der Physiologie des Nervensystems.*' *Biologisches Zentralblatt*, 1899, 19.

[4] This position is maintained in many of this investigator's numerous works. Cf., for example, 'Comparative Physiology of the Brain and Comparative Psychology,' 1899.

[5] Bechterew, W. von, 'Objektive Psychologie: oder Psychoreflexologie, die Lehre von den Assoziationreflexen,' German translation from the Russian, 1913, pp. iii *f.*

[6] Pawlow, I. P., 'New Researches on Conditioned Reflexes,' Science, 1923, N. S., 58, p. 359.

[7] The objections to physiological objectivism are well given by Yerkes, R. M., 'Objective Nomenclature,' *J. Comp. Neurol. and Psychol.*, 1906, and by Forel, A.,

cepted, the problem of the nature of sensation is, of course, solved by the clear, and possibly correct, method of denying the legitimacy or the significance of the question.

If it is felt, however, in spite of the difficulties of the structural view of sensation, that there is no valid reason for rejecting all study of mental life, we may still find alternative positions in contemporary psychology. In the following quotation, for example, Professor McDougall illustrates this fact with refreshing candor, when he says,

> Behaviorism seems to me to have arisen as a reaction against, or rather as a natural development of, what may perhaps be called Titchenerism. One often hears discussions in which it seems to be tacitly assumed that the student of human nature has to make his choice between two alternatives, behaviorism and Titchenerism. I venture to suggest that there remains a third alternative, namely, psychology. If I were asked to define Titchenerism in a few words, I would say—it is the abortive psychology which stultifies itself by refusing to attempt to deal with 'meaning and motive.' . . . Titchenerism, then, excludes from the province of psychology all awareness, all 'meaning,' all motives, leaving only meaningless clusters of sensations and images; behaviorism rightly seeing that these meaningless clusters are of no help to it in the interpretation of human behavior, throws them out of the window after the 'meanings.' Thus, Titchenerism and behaviorism are two stages of the same process by which the science of human nature is degraded into an arid waste of 'stimuli' and shadowy meaningless 'reaction patterns,' a world of 'moving shadow, shapes that come and go,' empty of all knowledge, value, motive, and meaning.[1]

Following the suggestion of the above quotation we may now turn to see how that discipline which Professor McDougall calls 'psychology,' but which we should, for the sake of specificity, prefer to denominate as 'the psychology of Dr. McDougall,' treats of sensation.

Like the mechanistic behaviorists Professor McDougall is fully sensitized to the difficulties of a psychology that considers only consciousness.[2] Such a conception of psychology leads almost at once, he insists, to atomistic analysis and then to a reification of abstract elements.[3] He likewise urges that

'Sinnesleben der Insekten,' 1910. The objections to the objective program for psychology as proposed by the behaviorists are well stated by Titchener, E. B., on 'Psychology as the Behaviorist Views It,' *Proceedings of the Amer. Phil. Soc.*, 1914, **53**, and by Roback, A. A., *op. cit.*

[1] McDougall, W. 'The Use and Abuse of Instinct in Social Psychology,' *J. Abn. and Soc. Psychol.*, 1922, **16**, note 44 on p. 329.

[2] *Idem*, 'Outline of Psychology,' 1923, pp. 16 *ff.*

[3] *Loc. cit.*

an atomistic description of behavior in terms of reflexes or other physiological elements is untenable.[1] Instead of dealing with either psychological or physiological abstractions, he contends that psychology should study what he terms 'mind.'[2] 'Mind,' as he conceives it, expresses itself in both experience and behavior; thus he says,

> . . . the sense qualities of experience resulting from the sense-stimulation is clearly not the physical object itself, but only a sign of it, an occasion for our thinking of the object; it is a sign of the presence of the object, a sign which may suggest it to the mind, or may set us thinking of it.[3]

But, if we ask what is the nature of the 'mind' that has these experiences, we do not have to search the writings of this student in vain for an answer. He tells us in an article in 1920 that,

> The sense impressions are normally transmitted through a hierarchy of monads undergoing further elaboration at each level, until they are reflected to the dominant in an highly elaborated form, conveying delicate spatial, temporal, and other meanings.[4]

These 'monads,' which seem at least to some extent to replace the nervous system which is considered by mechanistic psychologists, are very real to Dr. McDougall, for he continues,

> What is the relation of the dominant monad to heredity and evolution? Is he not only the ruler of his society but also the patriarch and progenitor? I do not see how we can avoid the conclusion that the monads are propagated by a process analogous to budding.[5]

The atomistic conception of sensation is thus rejected by Professor McDougall.[6] The alternative that is offered in his system of 'minds' and 'monads' for the shortcomings of the structural or behavioristic positions is, however, far too speculative for the demands of laboratory psychology.

If this 'mentalistic' psychology is inadequate to the task of resolving the dilemma so vividly suggested by Dr. McDougall himself between 'Titchenerism' and behaviorism, there

[1] *Idem*, pp. 28 *ff.*

[2] *Ibid.*, pp. 35.

[3] *Ibid.*, p. 226.

[4] *Idem*, Presidential Address, The Society for Psychical Research, 1920. *Proc. Soc. Psychical Res.* 1920, **80**, 116.

[5] *Idem*, p. 117.

[6] In the 'Outline of Psychology' there is a consideration of perception rather than sensation. See Chapter VIII.

are still other tendencies in modern psychology to which we may apply for assistance. The most important of these movements for our present study of sensation is that position which attempts to reduce the facts of bodily and mental activity to the single formula of response to stimulation. Thus, Professor H. C. Warren tersely defines sensation as, 'the central effect of stimulation, observed in ourselves,' [1] and Professor R. S. Woodworth says, "Sensation is a response; it does not come to us but is aroused in us by a stimulus." [2] As distinguished from the structuralists who study consciousness for its own sake, and the behaviorists who reject it as of no value to scientific study, these students for the most part, in the words of Professor Max Meyer, find that, "The scientific value of introspective psychology consists merely in the fact that it aids us in discovering the laws of nervous functions." [3] According to this view, the difficulty with the older notion of the relation of the reflex arc and the sensation as pointed out long ago by Professor John Dewey, is overcome.[4] Moreover, the present writer does not believe that this assumption even implies the acceptance of a prejudiced view upon the 'body-mind problem.' Rather, it means simply the acceptance of what can be termed the 'agnostic' position of Professor Thorndike, which he presented in the statement,

> Man's original nature is such that, when certain parts of his millions of neurons act in certain ways, he has, or is, certain states of awareness, feeling, consciousness, purely mental existence, or whatever one chooses to call the inner life. . . .[5]

According to this neural view, then, the activity of the organ-

[1] Warren, H. C., 'Human Psychology,' 1919, pp. 79*f*.

[2] Woodworth, R. S., 'Psychology: A Study of Mental Life,' p. 187.

[3] Meyer, M., 'The Fundamental Laws of Human Behavior,' p. 239.

[4] Dewey, J., 'The Reflex Arc Concept in Psychology,' PSYCHOL. REV., 1896, 3, p. 365. He says in part, "We ought to be able to see that the ordinary conception of the reflex arc theory, instead of being a case of plain science, is a survival of metaphysical dualism, first formulated by Plato, according to which the sensation is an ambiguous dweller on the border land of soul and body, the idea (or central process) is purely psychical, and the act (or movement) is purely physical. Thus the reflex arc formulation is neither physical (or physiological) nor psychological; it is mixed materialistic-spiritualistic assumption."

[5] Thorndike, E. L., 'The Original Nature of Man' (Volume I of the 'Educational Psychology'), 1913, pp. 170*f*.

ism as a whole,[1] the reaction arc,[2] the receptor,[3] the cerebral cortex,[4] or the change in muscle or gland [5] is emphasized, although in no system is any one of these agencies considered as solely accountable for sensation. Those, however, who study experience not for its own sake so much as for the light which it sheds upon otherwise inaccessible physiological activity, must, in this very study of the conscious functions, face all of the difficulties which confront the student whose only aim is mental description as such. In other words, introspection, even if considered as one method of *physiology*, is open to all the benefits and difficulties which face this same agency when used as the tool of structural psychology. The advantages and disadvantages, which are inherent in the products of introspective analysis, such as attributes or sensations are the same whether these elements be considered, as in the structural psychology, for their own sake; or, in the 'stimulus-response' psychology, as a clue to otherwise non-observable but peculiarly significant behavior.[6]

There are, however, other psychologists who, while interested primarily in the nature of consciousness, deny the validity of the atomistic analysis which is used alike by the structuralist and the 'stimulus-response' psychologist in the study of experience. Indeed, there is a growing tendency among certain writers to reject the whole conception of the psychical element which we have been considering in this paper. It must be clear at the outset that any conclusion upon this view of mental life is important, not only to the psychologist who is interested in the description of conscious states as such, but also for the student who is concerned with

[1] Cf. for this standpoint, Lewes, G. H., 'Problems of Life and Mind,' 1880, p. 83. "The sensorium is the whole which reacts on the stimulation of any particular portion of that whole."

[2] Cf. Dunlap, K., 'Elements of Scientific Psychology,' 1922, p. 208.

[3] Cf. Mursell, J. L., 'The Concept of Sensation,' *J. Phil.*, 1922, 19, 684.

[4] Cf. Smith, S. S., and Guthrie, E. R., 'General Psychology in Terms of Behavior,' 1921, p. 245.

[5] Cf. Givler, R. C., 'Psychology: The Science of Human Behavior,' 1922, p. 62.

[6] Paradoxically, however, it seems, at least to the present writer, that those who accept the products of introspection rather as a means than as an end, are more credulous than the better trained members of the structural school in the results which they record. Should the shoemaker stick to his last?

the data of experience as a clue to implicit physiological activity.

This tendency to question the value of psychological atomism is most explicit in a new tendency in psychology which may best be termed 'Configurationism.' Writers who support this point of view are not reticent in their claims. Thus Professor R. M. Ogden, who until recently considered the 'elements' as fundamental for psychology,[1] now says,

> Are we not ready to dispense with the element of sensation in systematic psychology? . . . One knows today that the complications and structure of experience cannot be analyzed into simple qualitative elements and thus built up by joining one to another but that one must work constantly with psychical forms. (*Gestalten.*)[2]

In a similar manner Professor Raymond Dodge, who until recently was willing enough to use the term sensation in his writings, although with qualification,[3] now says,

> . . . the traditional scientific accounts of sensation, perception, memory . . . tend to overemphasize the hypothetical invariants. . . . It is a great service of the modern German School to show that sensations as invariant elements of mental life do not exist, and would be valueless if they did exist. We long ago abandoned the hypothesis of mental faculties, but the spirit that postulated them still survives . . . .[4]

The members of this *Gestalt* or configurationistic school are, if we may judge by their numerous researches,[5] primarily interested in psychology as a description of consciousness, although they by no means limit themselves to this field. In conscious description, however, they use very different concepts than do, for example, the structural psychologists. Thus, Professor Wertheimer contends that a perception is in

[1] Ogden, R. M., 'An Introduction to General Psychology,' 1914. He says (p. 35), "We are now prepared to undertake an analysis of consciousness—to attempt a survey of the structural elements with which mind carries on its operations. The feature of consciousness that strikes us first and most of all is its power of revealing the world in which we live. Its revelations are based to a large extent upon elements known as *sensations*."

[2] *Idem*, 'Are There Any Sensations?' *Amer. J. Psychol.*, 33, 192.

[3] Dodge, R., 'Theory and Limitations of Introspection,' *Amer. J. Psychol.*, 1912, 23, 225.

[4] *Idem*, 'Problems of Human Variability,' *Science*, 1924, N. S., 59, p. 265.

[5] For a fairly complete bibliography up to 1921 see Koffka, K., *Psychol. Bull.*, 1922, 19, pp. 581–585. Since that date see especially the articles in the *Psychologische Forschung*, which is the organ of this movement.

no sense a 'bundle' of sensations, but that it is rather a totality in itself.[1] Each such totality is to be thought of, they urge, as a structure or configuration which has certain characteristics of its own, such as the fact that it may be transposed but not analyzed. Furthermore, it is held that if a so-called part is removed from, or added to, such a structure, not only the whole but the part is altered by the process.[2]

Indeed, Professor Koffka presents a program for the abolition of psychological analysis, and the substitution in its place of an integrative process.[3] But the analysis which this writer and Professor Wertheimer condemn [4] seems to pertain more to the one, as we have seen above, that is renounced by Professor Titchener as representative of associationism, rather than to true structuralism.[5] The following statement, however, in its suggestion of the limits of analysis is really significant for our present problem,

> The analytic method can, according to this way of thinking, only find out into what kind of reactions other reactions can be transformed by the change of attitude called psychological analysis.[6]

In other words, 'the sensation' is thought to be, according to this view, a methodologically produced artifact. The plausibility of this 'false analysis' rests, according to Professor Wertheimer, upon an invalid analogy between the methods of psychology and physical science;[7] while Professor Köhler contends that even in the physical world configurations are to be discovered.[8]

Turning now from the actual contentions of the configurationistic school upon those phenomena which have been called sensation, it will be expedient to look more closely at the

[1] Wertheimer, M., 'Unterschungen zur Lehre von Gestalt. I. Prinzipielle Bemerkungen,' *Psychol. Forsch.* 1922, 1, pp. 47 *ff.*

[2] Humphrey, G., 'The Theory of Einstein and the *Gestalt Psychologie:* A Parallel,' *Amer. J. Psychol.*, 1924, 35, pp. 353 *f.*, and Koffka, K., 'Introspection and the Method of Psychology,' *Brit. J. Psychol.*,1924, 15, p. 157.

[3] *Ibid.*, p. 153.

[4] *Idem*, 'Perception: An Introduction to the *Gestalt-Theorie*,' *loc. cit.*, p. 533. A conscious unit is taken to be a bundle of such elements (as sensation).

[5] Titchener, E. B., 'Sensation and System,' *loc. cit.*, p. 265.

[6] Koffka, *loc. cit.* in *Brit. J. Psychol.*, p. 155.

[7] Wertheimer, M., *loc. cit.*

[8] Köhler, W., '*Die Physischen Gestalten*,' 1920.

present status of atomism, not so much in the physical, as in the physiological realm. The present status of biological theory in regard to analysis is important for the problem of sensation from at least two angles. First, the analogy has been frequently drawn between sensation and various anatomical and physiological elements.[1] Second, the existence of isolated neural elements is an important assumption in certain of the reaction theories of sensation which have been considered above.

In recent biological literature there has developed, so far as may be discovered quite independently of the configurationistic movement in psychology, a tendency to criticize the consideration of parts without reference to the wholes from which they come.[2] Particularly significant from this point of view for our present problem is the work of Dr. C. M. Child upon the function and development of the nervous system.[3]

This investigator has come to the conclusion as a result of his experimental embryological and physiological studies that in all living material one must face two separate but interrelated questions, namely, the problem of the nature of material and the problem of the nature of pattern or form.[4] Furthermore, he seems to demonstrate that pattern is in no sense merely an intrinsic growth out of material, but rather that it is dependent upon a functional relationship between a given material and its environment.[5] Atomistic analysis therefore, while it may yield important facts in regard to material, by its very nature destroys some of those relationships upon which form depends. Thus, at best, such an analysis gives but a partial view of the totality which is analyzed. Finally, this experimentalist arrives at the conclusion that the organism may be regarded as

[1] Cf. Titchener, E. B., *loc. cit.* in Vol. I, Part 1, 'Experimental Psychology.'

[2] Cf. for example, Ritter, W. E., 'The Unity of the Organism,' 1919; Sherrington, C. S., 'The Integrative Action of the Nervous System,' 1906; Haldane, J. S., 'Mechanism, Life, and Personality,' 1914.

[3] Child, C. M., 'The Origin and Development of the Nervous System from a Physiological Viewpoint,' 1921, *Idem*, 'Physiological Foundations of Behavior,' 1924.

[4] *Ibid.*, pp. 1–4.

[5] *Ibid.*, pp. 6 *ff.*

> . . . representing essentially a behavior pattern, arising in protoplasm in response to its relation with the external world, and particularly the relations concerned in excitation and its effects.[1]

Therefore, returning to our analogy between the atomistically considered sensation and the single cell, is it not possible that all is not quite so clear as the psychological structuralist would have us believe? For, if we accept the view in biology suggested above, must we not grant that there is no reason to believe that the cell can ever be understood, save incompletely, away from the tissue, or the tissue away from the individual, or the individual away from the environment?[2] If this be the case, it would seem that this sensation-cell analogy would be one that would appeal to the configurationist more than to the structuralist, who first chose it as an illustration. We must note at once, however, a difference between the configurationists' denial of atomism and the modern biologists' contention that atomism, while valuable, is nevertheless not adequate to all phases of any descriptive problem. The researches of these physiological students amply testify to the fact that they do not *reject* atomistic analysis because of its inherent destructiveness of the whole-part relationship, but rather do they try to *interpret* the results which such analysis produces in the light of the intrinsic limitations of the method.

IV. Conclusions

In this biological position which we have just considered there seems to be an important truth for psychology, and particularly for the theory of sensation. If we accept the biological definition of the organism just cited, namely, that it is a behavior pattern arising out of a protoplasm, historically considered, then it seems to follow that the physical and the mental life are but two aspects of the same series of events. However, paradoxical as it may appear, possibly because of what Professor James has called our 'partial mindedness' we

[1] *Ibid.*, p. 268. This essential interrelation, particularly in the nervous organs between the whole system and the individual cells and cell groups, is also made clear in the work of Kappers (cf., for example, 'A Study of the Phenomenon of Neurobiotaxis,' *J. Comp. Neurol.*, 1918, 27,) and Detwiler, (cf., for example, 'Experiments on Transplantation,' etc. *J. Exper. Zoöl.*, 1923, 37.)

[2] Child, *op. cit.*, p. 1 *ff.*

must, it seems, violate this very totality by a destroying analysis if we are to gain any scientific conception of the true significance of the whole.

But (and here seems to lie the crux of the whole matter) *this analysis and its products should always be regarded strictly in the light of the essential and intrinsic limitations of the method.*

From the standpoint of this conception of the process of analysis many of the difficulties that have been encountered in our review of various systematic positions upon sensation disappear.

In the structural system we saw that the problem of the relation of sensation to its attributes remained insoluble so long as we attempted to understand it in the light of the 'bundle hypothesis.'

Under this new conception, however, it seems that this relationship is but a special case under the generalization that in psychology any true form is more than the sum of its parts. But, we may ask, is the sensation directly observable, and if so, is it a valuable descriptive tool? These are questions which can only be answered by further experiment. If we hazard a guess, however, it would be that, instead of occupying as at present in certain systems a position of supremacy, the integration called 'a sensation' may come to be recognized as merely one of the mental functions which are to be described in attributive terms. If this be the case, the second difficulty which we saw in the structural view of sensation, namely, that it imperfectly separates sensation from image and affection, disappears. The attributes, experiments indicate, are continua; and so it seems but a step to the contention that the integrations of these attributes, while possibly conveniently called by specific names in their 'central tendencies,' are in reality to be arranged so that they will join by imperceptible nuances.

If we turn back to the behaviorists, we may remember that these students pay at least 'lip reverence' to the organism as a whole. Indeed, upon the principle of the unified individual hangs the only consistent distinction which the members of this school can make between the discipline which they confess

and physiology. In spite of this fact, the writings of avowed members of this school often disclose a rather uncritical atomism of a physiological sort. From the point of view we are here suggesting this neural atomism is valuable enough, provided that its intrinsic limitations are always remembered. Only, contrary to the radical teachings of the members of this school, the present writer can see no valid reason why the same guarded procedure that is used for physiological response, should not be used for the analysis of mental life under the rules of an enlightened and newer structuralism. For in this way, at least for the present, certain activities otherwise closed to research are opened to direct self observation.

Turning to the mentalistic school, as represented in the psychology of Professor McDougall, we see a ready agreement that the atomistic physiological and psychological analysis is incomplete. Instead, however, of facing the mechanistic fact that this proceeding destroys the functional relationships which are the pattern of the totality, it urges rather the intervention of some external ordering principle. The 'mentalstic' view therefore leads not to the hard work of the experimental physiological and psychological laboratories, but rather into such easy substitutes as the postulation of a patriarchism of mysterious monads.

In conclusion, then, we may say that, according to the position here tentatively proposed, psychology must continue to talk of reflex arcs and simple mental functions and to attempt the description and correlation of these appearances in accurate quantitative terms under controlled experimentation. This standpoint, however, further leads to the insistence that the end-products of this analyses have no final 'thingness' about them, save in their normal relationships. In other words, the conception of sensation, or that of any other abstraction, is important for psychology in so far as it helps partial minded man to grasp something of the significance of that really total *gestalt* of a behaving organism in an environment considered in its most pervasive sense.

LAPSES INFLUENCED BY SIMILARITY

BY L. W. COLE

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At the recent meeting of the psychological section of the British Association it was stated repeatedly that we are very much in need of a neurological theory to account for the ordinary lapses of tongue, pen, and typewriter. I had naïvely supposed that the facts adduced by Sherrington in his demonstration of the final common path were adequate to explain all lapses not due to similarity: namely, omissions, transpositions, anticipations, and repetitions or reduplications.

Turning to the philologists, we find the following psychological statement, apparently approved by Stern or derived from his writings. "During the utterance of any word the memories of movements necessary to produce the single sounds [1] of which it is composed pass successively through the focus of consciousness. But the memories which have passed through this focus faintly abide in consciousness for some time, until they have entirely faded from it; and the memories of the movements necessary for subsequent sounds faintly arise before they enter into the focus. The memory of the sound that is being uttered, memories of sounds just uttered, and memories of sounds presently to be uttered, are simultaneously present in consciousness, though in varying strength. If now at any time an element of this undercurrent of sound memories fails to retain its subordinate position and either enters the focus of consciousness before it should, or re-enters it after once leaving it, an interference with the

[1] Note that 'loss of memories of the efforts necessary to pronounce words,' such memories as those mentioned above, is recognized as the cause of motor aphasia. (M. Allen Starr, 'Familiar Forms of Nervous Diseases.') It is a curiosity of our teaching that a recent most widely used text-book of psychology states that 'motor aphasia is due to disturbance of the motor region.' If this were so, there would be paralysis of the vocal organs in every case of motor aphasia. Must students be given simple statements that are untrue? Has simplicity become so valuable as that?

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normal form of the word results." (Hans Oertel, 'Lectures on the Study of Language,' p. 225.)

This is an excellent psychological account except perhaps that it ascribes too much to consciousness. Many of our errors are unconscious. Would it not be truer translated into physiological terms, thus? During the utterance (or writing) of any word a number of neurons or nerve paths of a higher order 'converging' (Sherrington) upon the final common path to lips or hand stand ready to discharge their impulses into it. If, for any reason, the impulse next in order is delayed, its immediate or a more remote successor may discharge its impulse instead, due perhaps to the intensity of its excitement. Thus would occur an omission and 'anticipation' which is probably the most frequent type of lapse. The proper impulse may then discharge, causing transposition. Or one neuron may discharge twice, repetition or reduplication. This would all be due to the fact that the final common path must transmit impulses from the higher converging neurons in succession; not simultaneously as does occur in the 'scrambled' telephone and telegraph messages of phantom circuits. This would hold true of the utterance of a word or a whole sentence, in which case syllables are most often omitted or transposed, or it may hold of writing in which the common omissions are words, or of typing in which particular letters of the alphabet are omitted or transposed, or of memorized matter in which the commonest omission is a paragraph or a stanza.

Since even neurons of the second order have many of their activities below the conscious level, consciousness often has little to do with the process. Often we find our omissions or transpositions in a letter only after re-reading it. In fact, the narrowness of the focus of consciousness may be simply a necessary adaptation to the narrowness of the final common path. A broad but intense focus would have the result of releasing more impulses from neurons of the highest order so that, as convergence grew narrower, there would be a 'traffic jam' and lapses would be more frequent than they are. As writing is necessarily far slower than speech, lapses occur

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more frequently in the former, but any attempt to hasten either speech or writing increases the number of lapses. Philologists recognize hasty speech as one cause of phonetic changes. This seems to me a fair translation of the philological account of lapses aside from the more subordinate rôle ascribed to consciousness. It is observable that we are unconscious of most of our omissions.

Examples are innumerable. We type *fi* for *if* or *adn* for *and*, to illustrate only with short words. Just now I suggested that a letter be addressed to Emporia. Two minutes later the listener inquired, "Did you say Peoria?" Thus the latter sounds of the word were correct, the sound of *p* anticipated to the beginning of it, and the *m* omitted. Should we assume that perhaps four neurons of the second order stand ready to discharge their impulses in order into the final common path in pronouncing such a word, we would have a physiological reason why any syllable might be omitted or anticipated, any two transposed, or, what is less frequent, any syllable reduplicated. The *m* above is plainly omitted because it is not consonant with the sounds of the second word. Introduction of a syllable is rarer still, yet it might be due to a mere lapse that 'sco' became the sign of an inceptive verb, etc. If my 'translation' into physiological terms is a fair one, be it noted that nothing is employed or implied which is not proved in Sherrington's demonstration of the final common path.

Which neural path shall discharge first is often determined by the number of times it has previously discharged into this final common path, but it is often influenced by similarity. Both influences are observable in producing this headline which I have clipped from a New York newspaper. The subhead reads, 'Natural History Museum Artists Descended in a *Suburban* Tube in the Bahamas.' The writer of this had doubtless spoken or written the word, 'suburban,' many times, the word, 'submarine,' far less often. That the error got into the paper shows that it was made almost reflexly.

The type of one's errors is changed if he turns to translating or copying. I was about to translate the word 'détruit' by the

English word 'destroyed.' I wrote 'det'—then necessarily stopped. This was plainly due to the word before my eyes, or rather which had just been before my eyes. In other words, higher neurons from a visual-image area of the cortex were discharging downward toward the final common path rather than from the auditory-image area, as occurs in spontaneous writing or in writing from dictation. This change to a visual type of mistake shows, as Sherrington points out, that many different types of neurons (or centers) must use the same final common path.

Two or three of his statements seem adequate to explain the types of lapses named above. "The final common path must be used in succession, not simultaneously, by the higher neurons discharging into it. Its rhythm must be equal to theirs." "Before finally converging upon the motor neuron the arcs converge to some degree" (117), and "the threshold of each succeeding reflex is lowered by the excitation just preceding its own" (p. 184).

It is objected rightly that some lapses cannot be explained by this mechanism. For example, the phrase 'to write a bicycle' is an actual case. This involves similarity of sound and I think it demands the assumption that certain patterns exist in the higher centers of the brain. In fact every association by similarity, I think, demands this assumption if it is to be explained at all. A simple recall of my own, whose correct completion required two minutes, will illustrate. I was endeavoring to recall the name, *June Ashley*. Instead I first recalled the name, *June Downey*. The similarity is so great and the recall so simple that I think a cortical pattern must be assumed not much more complex than the following diagram.

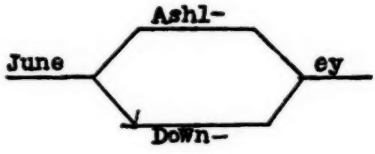

FIG. 1.

The headline quoted above would seem to be due also to just such a pattern.

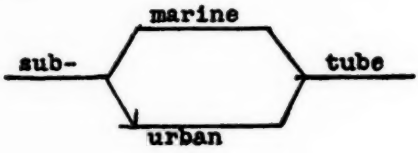

FIG. 2.

Such a conception is merely extending the principle of the common path to account for association by similarity. If our talk about nervous impulses discharging along neuron chains in the brain has any real meaning, and association by similarity is a fact and not a phrase, some such hypothesis as the above must be made and this seems to me the simplest one adequate to account for the facts.

The evidence for this is as follows:

1. There are common paths in the brain and nervous system, the final ones in neurons of the first order. In all probability there are also associative common paths. Without them it is hard to conceive how association other than the serial type could ever occur.

2. There are structural patterns, first in the retina, an outgrowth of the brain, and second, in the visual occipital cortex. Head and Lister (*Brain*, 1916) have worked out the visual patterns of the visual area, showing the representation of each quadrant of the retina on each portion of the occipital cortex. It is not the simple, geometrical projection implied by the ordinary figures of the retina, chiasma, and occipital area, but it is a projection in which each point of the retina is represented by a point in the cortex. If one should look a second time at the same object with both retinas at exactly the same points of view, the same patterns of rods and cones would be stimulated and the same cortical pattern re-aroused to activity. In the same way, the somaesthetic area is a projection area. (Barker.) If, therefore, the afferent and efferent cortex consists of projection areas, a pattern of which

is active when stimulation occurs, it is hardly likely that the pattern principle stops abruptly at the boundary of these areas and does not extend at all to the association cortex. Except for more complex folding such patterns might be as easy to discover as projection patterns.

3. Perhaps the most important evidence is that the conception above stated would result in the agreement of logic with observed fact. It was long ago pointed out that one idea cannot aid in the recall of another by similarity, because the second idea is not yet present in consciousness, even assuming that ideas could be similar when present. "The similarity of two things does not exist till both things are there" (James, I, p. 591). But if the ground of similarity were two brain patterns with a partly common path, as in the simple figures above, the similarity or its ground would be 'there' always in the common paths of the two patterns. The moment nervous impulses discharged along both the common and non-common portions of the two patterns both the similarity and the difference, say of two names recalled or two lines of a poem, would be recognized. If I may assume this as an explanation of my own recall, discharge along the common portions and the lower portion led in two minutes to discharge along the upper portion and to the consequent recall of the name I sought. Hence it is that in attempted recall we do not recall simply nothing at all but some item more or less similar which, if kept focal, results in recall of the sought-for item. It has always been recognized that similarity is a genuine cause of recall and of productive thought, because it is an observed fact. Historically, it is the second important discovery made in psychology, I believe. This has also been recognized as logically absurd so long as psychology talked of the association of ideas by similarity. Of similarity, James says: "It is meaningless to talk of it as an agent productive of anything, whether in the physical or psychical realms. It is a relation which the mind perceives after the fact." Plainly a relation cannot be an agent. But if two cortical patterns may have a common path or common paths, the discharge of nervous impulses through the patterns

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may be an agent of the production of ideas, exactly as it is in the case of the habitual connection of neurons and habitual recall. Similarity would thus account also for original thought as it always has done.

4. At the meeting of the British Association Parker showed to the satisfaction of the physiologists that nervous activity results in the release of CO_2. It therefore implies oxidation. But oxidation follows chemical laws and in a network or a felt-work it must necessarily trace a pattern, 'like sparks in burnt-up paper,' to quote Lowell's phrase. Even before Parker's proof it was believed that nervous impulses which come upon oxidized atoms must pass across other synapses than those they would have crossed had no obstacle been present.

One objection to the hypothesis proposed will doubtless be that it is so obvious that it is superfluous to state it. This cannot be the case, else the psychologists present at the Toronto meeting would not have said that we are greatly in need of a neurological explanation of lapses. Nor would they have rejected Sherrington's work as an explanation of the lapses which are uninfluenced by similarity.

A second objection will be that our conception is far too simple to account for the wonderful complexity of the brain and its action. True, but does it offer a beginning; and if the common paths of patterns were numerous and many of the patterns complicated, what degree of complexity could they not match? The infinite complexity of matter is now accounted for by the rather simple patterns formed by the orbits of electrons. If the conception proposed is simple, it merely conforms to the law of parsimony.

A third objection may be that it is incapable of proof. Yet its proof or disproof seems even more possible than the measurement of the rate of propagation of the nervous impulse which was made only eleven years after the most eminent physiologist of those times said that its rate could never be measured.

However, a working hypothesis is tested by the value it might have if it were true. This conception would bring all

the types of lapses under the formula of the common path. Association by similarity would find a reasonable explanation. Most poetry would be accounted for, and the student of literature who remarked that poetry is like lace, would have come nearer to a flash of genuine insight than to a figure of speech. We should really have a standard of measurement of the claims of the *Gestaltpsychologie*. More important than this, the conception would throw light on the question of the consciousness of relations as elements, a claim admitted by one group of psychologists and denied by as many in the opposite camp, who give a physiological explanation dependent on the body and almost exclusive of the cortex. Finally, recognition would find an explanation better than the current ones.

We describe habit and habitual associations in terms of discharge of impulses along neuron chains exactly as if we had seen the nervous impulse leaping across the synapses made most permeable by previous repetition. On the other hand, association by similarity has been eliminated from educational psychology as not a factor in the economy of learning. This elimination will hardly gain the assent of thoughtful observers. The conception here proposed may restore similarity to its rightful place in our science. The insistence that there is no difference *at all* between human and animal learning hardly does justice to the facts. Finally, our conception may be compared with the more magical interpretation of lapses by Freud. His appeal to sex is hardly stronger than his appeal to the appetite for magic which seems undiminished since the days of witchcraft.

It is to be hoped that lists of very simple recalls strongly influenced by similarity may be made, to see if they give evidence for or against the explanation by means of partly common paths.

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STATISTICAL CONSIDERATIONS IN INTERPRETING THE EFFECT OF TRAINING ON INDIVIDUAL DIFFERENCES

BY J. CROSBY CHAPMAN

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In a recent article [1] on 'The Effect of Training on Individual Differences,' Reed, after manipulating his own data and the data of other experimenters, comes to the conclusion that, at least in fairly simple functions, the effect of training is to equalize rather than to increase individual differences. The generally accepted conclusion that training tends to increase individual differences is due, he claims, to the faulty use of interpretative statistical devices. He makes the statement, "If we measure improvement by increase in the amount of work per unit of time and individual differences by the absolute amounts of the increase, then the [generally accepted conclusion is] justified, but as soon as we measure improvement by decrease in the amount of time per unit of work and individual differences by the absolute amounts of the decrease, then we are justified in drawing diametrically opposite conclusions. I presume that all will agree that amount of time per unit of work is just as accurate a way of measuring efficiency as amount of work per unit of time. But the two methods lead to opposite conclusions about the effect of equal practice on improvement and individual differences." The author then proceeds: "It is the purpose of this paper to show that since this is the case, neither of these methods are reliable, that [the generally accepted conclusion is] therefore false, and that when individual differences are measured by more accurate methods we must conclude that equal practice decreases individual differences."

In short, the thesis that Reed maintains is that the ratio of performance of the slow group to the fast group before and

[1] J. Exper. Psychol., 1924, 7, 186–200.

after the practice period is the only legitimate way in which to discover the manner in which training affects individual differences.

Reed presents the following two tables to show the inconsistency of two common statistical procedures:

TABLE I

Subject	Score First Minute	Score Last Minute	Gain
A	5	15	10
B	12	30	18

TABLE II

Subject	Seconds per Example First Minute	Seconds per Example Last Minute	Gain
A	12	4	8
B	5	2	3

The last columns in which the gains are recorded indicate the obvious discrepancy between the two methods of investigating the question and lead to diametrically opposite results. The author then asks, "Where is the nigger in the woodpile?" He then proposes that the most satisfactory measure of the effect of training on individual differences is obtained by using the ratio of scores at the beginning and at the end of the practice of the initial slow and initial fast individuals. When this measure is applied to hitherto published results it shows that individual differences are decreased by training.

Before this procedure can be accepted it is necessary to inquire carefully into the nature of the scale and the nature of the units that measure the mental function which is under consideration. Certainly before the ratio of the performance of the slow and fast can be accepted as a legitimate measure, we must question the meaning of the zero of the scale. While a score of zero represents no power to perform a mental function with units of the degree of difficulty presented in the test, if we consider all the neural connections possessed by an individual which contribute to the ability in the mental function and yet just fail to permit him to score, they are

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legion in number. In the sense of measuring initial ability, an actual zero on the test does not mean zero of abilities possessed.

In considering the effect of education upon different degrees of initial ability, we are not interested in the ratio of the performance of the low initial scorers to the high initial scorers before and after a period of practice unless we are sure that our measure of initial ability represents as nearly as possible an individual's true standing and not his standing on an arbitrary scale with a purely arbitrary zero.

Of course, any worker if he so wishes may define his particular problem in any way that he desires. Thus, in the case under consideration, Reed may claim that he restricts his interest and study to the investigation as to whether the ratio of performance of the initially low and the initially high increases or decreases as a result of practice when performance is measured by a scale with a purely arbitrary zero. No one can quarrel with the results obtained, though one may question the value and significance of the study. But one has every right to ask whether the conclusions drawn from data based on a scale with such an arbitrary zero can be applied to the larger question as to 'the whole problem of heredity and environment.' In this larger question of the effect of training on individual differences, if we are going to employ any procedure such as Reed suggests, using the ratio of scores of the fast and the slow workers, it is essential that we have an initial score that represents true ability on an absolute scale where zero ability is not arbitrary. If we fail to interpret our figures in the light of the arbitrariness of the zero, we land ourselves in an absurd position when we use the ratio method of Reed. Thus employing an arbitrary scale with an arbitrary zero, the individual, who improves from the arbitrary zero to the first point of a scale in a small period of practice, has by the ratio method improved infinitely. This is the *reductio ad absurdum* of the procedure. In the attempt to measure the effect of education upon different levels of ability, any scale with an undistributed zero, in fact any of our usual arbitrary scales in psychology and education with their arbitrary zeros yield

quantitative results that cannot be handled by any ratio procedure.

If we are to get a true measure of initial ability, we must allow for the fact that any zero score is arbitrary. That this is a necessary correction will be apparent from the following illustration. Reed himself quotes data from improvement on the Thorndike-McCall Scale. Now a zero on the Thorndike-McCall Scale represents an ability in reading anything from that possessed by an amoeba to that held by a brilliant Galton at the moment just before he could answer the most simple element. That is, the zero is not a true zero but an undistributed zero and an arbitrary zero. If we are interpreting the data to investigate the problem of heredity and environment as Reed proposes, it is purely arbitrary to regard zero on this particular scale as necessarily representing zero ability in the function in question.

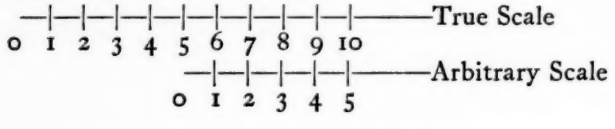
Suppose, in the attempt to think a little more nearly in terms of some absolute scale and less in the terms of the arbitrary scale, we make a few assumptions. Let us suppose that an initial score of 8 in, say, 60 seconds in any test means that the individual has operated $N_0 + 8u$ contributory factors.[1] This would mean that the individual who just failed to score on the scale was capable of operating just short of $N_0 + u$ contributory factors. Furthermore, until $N_0 + u$ contributory factors are operated in 60 seconds, no score is recorded on the arbitrary scale. In view of the fact that we are dealing in the ordinary mental function with vast numbers of these factors, we may further assume that for the smallest contributory-factor-count in which the experimenter would be interested the average factor contribution to effectiveness in the mental function, as measured on an adequate scale, would be the same. This assumption will enable us to speak of $N_0 + u$ as a score.

A physical analogy, if it is not pressed too far, may be useful to give meaning to the term N_0 in a test given under the usual time limit condition. The temperature scale which so

[1] Because we know so little of the physiological factors involved, I prefer to use the vague term 'contributory factor' rather than a term such as neuron, synapse, bond or connection. Certainly, improvement often means the disappearance of certain inhibitory neuronic activity.

naturally suggests itself is not pertinent because in such a scale the factor of speed does not enter explicitly. Rather we may take as our analogy a man rowing in a boat against the current of a river which flows at a speed of five miles per hour. Furthermore, we may assume that by means of a cog-wheel arrangement, such as that employed on funicular railroads, movement with the current is impossible. Then until the rower maintains a speed of just above five miles per hour in still water the boat will not move. To an observer on the bank his 'score' reckoned in miles per hour will be zero. Now suppose two rowers *A* and *B*; *A* rows at six miles per hour and *B* at eight miles per hour in still water. To an observer on the bank the speed of *A* will be 1 mile per hour and that of *B* will be 3 miles per hour. Apparently the ratio of the performances will be 1 to 3, whereas the true rate will be 6 to 8. In this river analogy the speed of the stream corresponds in the test situation to the degree of integration required before the individual can score. Furthermore, in the case of the rower, an amount of effort below a minimum point will not move the boat *however long* may be the period over which this effort is expended. This corresponds to the test situation. Here an individual of lower integration cannot merely by working a longer time produce the same score as an individual with the higher integration who is capable of scoring on the test.

A test of increasing difficulty in which the subject works to his limit irrespective of time would be represented on the river analogy by a rower working in a stream of opposing current where the force of the opposing current gradually increased the further he went. Under these conditions the final point reached in the stream would represent the ability. Now in this case the efficiency of the rower will only be truly expressed provided we measure from a point in the stream of absolute zero, namely, the place of no current, where theoretically the least energy put forth will produce the forward movement allowed by the cog-wheel system.



For example, if in the top diagram, o represents the point of zero current and successive positions represent successive impedances, then irrespective of the speed at which the rower negotiates his boat in relatively quiet water, we can measure his maximum capacity by the point on the scale that he finally reaches. But suppose, on the other hand, our arbitrary scale starts from the point shown on the lower scale, then two rowers who reach 2 and 1 respectively on the arbitrary lower scale will not have capacities in the ratio of 2 to 1, but rather in the ratio of 7 to 6.

Returning to the main issue, using the terms N_0 and u we may rewrite [1] Reed's first table even though we have no knowledge of the value of N_0 and u.

TABLE III

Subject	Initial Ability	Final Ability
A	$N_0 + 5u$	$N_0 + 15u$
B	$N_0 + 12u$	$N_0 + 30u$

The fact that N_0 and u are unknown prevents any simple use of ratios. For the purpose of investigating the effect of training on individual differences, the ratio of the initial ability is $(N_0 + 5u)/(N_0 + 12u)$ and not as Reed suggests 5/12; similarly the ratio of final ability is $(N_0 + 15u)/(N_0 + 30u)$ and not 15/30.

We have shown that the ratio procedure of Reed breaks down because N_0 is an unknown. It remains to show that the more orthodox method of treating data as used by Thorndike [2] and others still applies even though we refer measurements to an absolute scale rather than to the arbitrary scale. Thorndike claims that in several mental functions the more gifted individuals profit more absolutely than the less gifted. This entails (1) that the more gifted individual can be distinguished

[1] We do not know the degree to which N_0 is exactly the same for all subjects, nor do we know the meaning of u. The essential problem is to decide whether for individuals scoring 5 and 12 respectively, on an arbitrary zero scale, their efficiency measured on an adequate scale would be more accurately represented by $N_0 + 5u$ and $N_0 + 12u$, or by the simpler relation $5u$ and $12u$. It is the fundamental thesis of this article that the former hypothesis gives by far the more accurate representation of the true state of affairs.

[2] Educational Psychology, Vol. III, p. 305.

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from the less gifted individuals; (2) that the gains can be measured. In view of the fact that N_0 is merely an additional factor in all scores, whatever may be its value, one can easily differentiate the gifted from the less gifted by gross scores on any arbitrary scale. Moreover, the measure of improvement (final score minus initial score) introduces the cancelling term $(N_0 - N_0)$. The only assumption that this method makes is that the units at different points of the scale are equal. In our terminology this would mean that u is the same for units at any part of the scale.

The error which arises from neglecting the zero of the scale can be illustrated by Reed's two tables. Suppose we assume arbitrarily that the value of N_0 for the scale is 100 and that u is 1, the two tables therefore become:

TABLE IV

Subject	Initial Score Scale with Absolute Zero	Final Score Scale with Absolute Zero	Gain
A	105	115	10
B	112	130	18

TABLE V

Subject	Seconds per Unit of Performance on Scale with Absolute Zero	Seconds per Unit of Performance on Scale with Absolute Zero	Gain
A	.572	.522	.050
B	.536	.462	.074

In both cases it will be noted that the rapid individual gains the most, whereas if $N_0 = 0$, that is, if the arbitrariness of the zero is forgotten, the two methods yield opposite results as shown in Tables I and II.

If the main contention of this article is sound, it indicates that in measuring the relation between initial score and improvement, the more usual procedure of correlating initial scores with absolute gains is legitimate, while correlating initial scores with percentage gains is bound to yield equivocal

results. Furthermore, it indicates that any procedure in which a ratio of two scores is employed is legitimate only where zero on the test indicates an absolute zero of the ability tested. It once more issues the warning against the simple arithmetical manipulation of scores as true measures of ability.

As we have already suggested, it would scarcely have been necessary to point out the shortcomings of Reed's methodology had he not applied his statistical deductions to a problem concerning which there is a lively, if not bitter, debate in progress. In view of what has been stated, he has no right to draw the following conclusion: "Coming now to the general question of whether heredity or environment is the greater factor in causing individual differences it seems that we must be less enthusiastic about heredity than we have been."

It appears to the writer that the same theoretical point was involved in the discussion of growth and intelligence by Freeman [1] and Peterson.[2] Here again discordant results are obtained according as the product per unit of time or the time per unit product is used as the basis of measurement. If we reproduce part of their table, we get the following results:

TABLE VI

	Age				
	8	9	10	11	12
Time	42	42	42	42	42
Standard Score	40	52	64	78	88
Product per unit time	.95	1.24	1.52	1.81	2.10
Time per unit product	1.05	.81	.65	.55	.48

When product per unit time is employed the increments are equal; where, however, time per unit product is used as the basis of measurement, there is a gradual diminution in the increment. When the arbitrariness of the zero is considered, with corresponding meanings for N_0 and u, Table VI becomes:

[1] *J. Educ. Psychol.*, 12, p. 148.
[2] *J. Educ. Psychol.*, 12, p. 155.

TABLE VII

	Age				
	8	9	10	11	12
Time	42	42	42	42	42
Product on true scale	$N_0 + 40u$	$N_0 + 52u$	$N_0 + 64u$	$N_0 + 78u$	$N_0 + 88u$
Product per unit time	$\frac{N_0}{42} + .95u$	$\frac{N_0}{42} + 1.24u$	$\frac{N_0}{42} + 1.52u$	$\frac{N_0}{42} + 1.81u$	$\frac{N_0}{42} + 2.10u$
Time per unit product	$\frac{42}{N_0 + 4u}$	$\frac{42}{N_0 + 52u}$	$\frac{42}{N_0 + 64u}$	$\frac{42}{N_0 + 78u}$	$\frac{42}{N_0 + 88u}$

Owing to the fact that $N_0/42$ is a constant the increment remains the same on the product per unit time basis. But, when time per unit product is the basis, it will be seen that we must have a knowledge of N_0—a quantity which is unknown and which, unless we can in some manner get *en rapport* with nature, must, it seems to the writer, remain unknown. The value of N_0 is a function of all the contributory factors available for action which have been formed from the moment of impregnation. Because time is counted on a scale with a true zero, whereas all educational products are scored on scales with arbitrary zeros, I would disagree with Freeman when he says, "The theoretical question whether it is better to express growth in terms of absolute increments of score or relative decrements of time seem to me one which is open to debate." We can divide $(N_0 + xu)$ by t with meaning to gain knowledge of an increment because time is expressed in units from an absolute zero, but we cannot divide t by $(N_0 + xu)$ because N_0 is unknown and unknowable.

The last point brings us to the further statement made by Reed: "I presume that all will agree that amount of time per unit of work is just as accurate a way of measuring efficiency as amount of work per unit of time." What has already been said in this article is pertinent in this connection. Suppose in Table VIII, Columns 1 and 2 represent a distribution of ability on a certain test measured on the basis of product in a certain time; this shows a fairly normal distribution (Fig. 1). Now suppose instead of plotting in these terms we plot in terms of time per unit of work. Under these conditions the

distribution curve of time scores is notably skewed as the values in Column 3 would indicate (Fig. 2). The curve ex-

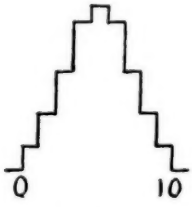

FIG. 1.

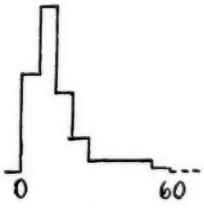

FIG. 2.

FIG. 1. Product per unit time, Arbitrary Scale
FIG. 2. Time per unit product, Arbitrary Scale

hibiting the skewness is obtained, it will be noted, when the time factor is divided through by the units scored in this time on the arbitrary scale. When, however, as previously suggested, we add on the factor N_0 and make in each case a score of $N_0 + x$ instead of x, it will be noted that Column 1 and Column 4 still give the same distribution as Column 1 and Column 2, only in this case the whole distribution is shifted upwards on the scale (Fig. 3). Similarly, it will be noted that

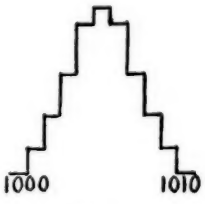

FIG. 3.

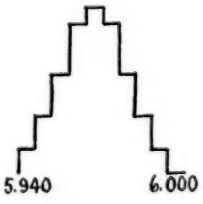

FIG. 4.

FIG. 3. Product per unit time, Hypothetical Absolute Scale
FIG. 4. Time per unit product, Hypothetical True Scale

whereas Columns 1 and 3 gave a most marked skew distribution, Columns 1 and 6, if N_0 be assumed to be as great as, say, 1000, give once more approximately a normal distribution (Fig. 4). The greater the value of N_0 the greater the normality. Even though *a priori* considerations [1] show that there

[1] Venn's 'Logic of Chance,' p. 39.

TABLE VIII

Freq.	Arbitrary Scale		True Scale		
	Product per 60 secs.	Time per unit product	Product per 60 secs.	Assuming $N_0 = 1000$	
				Product per 60 secs.	Time per unit product × 100
0	0	∞	N_0+0	1000	6.000
3	1	60	N_0+1	1001	5.994
7	2	30	N_0+2	1002	5.988
12	3	20	N_0+3	1003	5.982
18	4	15	N_0+4	1004	5.976
20	5	12	N_0+5	1005	5.970
18	6	10	N_0+6	1006	5.964
12	7	8.6	N_0+7	1007	5.958
7	8	7.5	N_0+8	1008	5.952
3	9	6.7	N_0+9	1009	5.946
0	10	6.0	N_0+10	1010	5.941

is nothing sacred about the normal probability distribution, we should certainly expect time per unit product when product is adequately measured on an absolute scale to yield an approximately normal distribution. The fact that in most reaction time experiments in psychology and education the skewed distribution is obtained is due primarily to the limitations in the scale itself, especially to the arbitrary zero which is characteristic of most psychological measurements. In view of the fact that both the original and the corrected products per unit of time give normal distribution, whereas the time per unit product only gives a normal distribution when corrected for arbitrariness of zero, it would perhaps seem that we have to make less doubtful assumptions if we measure traits by amount of work per unit of time rather than amount of time per unit of work. We may, therefore, conclude that amount of time per unit of work is as accurate a way of measuring efficiency as amount of work per unit of time *only when the amount of work is measured on an absolute scale.* In view of the fact that we can never discover what is the absolute scale, it is essential, if anomalous results are not to be obtained in studying such questions as increments of ability, to use as the basis of measurement product per unit time.

WHAT CONSTITUTES AN EMOTION?

BY DAVID WECHSLER

New York City

There are few subjects upon which more has been written in the last few decades than the emotions. Yet it is remarkable how much disagreement exists as regards the precise connotation of the term. There is, in fact, no generally accepted definition of emotion such that a reader may feel sure that the phenomena intended to be included by one author are exactly those intended by another.

Many reasons might be given for the uncertainty regarding the precise connotation of the term, but there is one which to my mind is fundamental. It is the insufficiency of the current descriptions of the actual phenomena which are said to constitute the psychophysical reactions summed up by the word 'emotion.'

The phenomena which have usually been distinguished as constituting the various aspects of emotion are: first, the perception of some unusual or threatening fact; second, some bodily changes; third, a subjective state or affect. And, depending upon the theory adopted by the author, the bodily changes are conceived as preceding, accompanying, or following the subjective state.

Now, the above description of the various aspects of emotion, while correct in a general way, simplifies the facts altogether too much. The result is that the same term is often used to include essentially distinct aspects of the phenomenon. And this has produced an ambiguity of terminology which has made unequivocal discussion of any theoretical question almost impossible, as can readily be seen by examining any of the more important current theories; for instance, that of James and Lange. Let us confine ourselves to James's enunciation of the theory and we shall quickly see that despite its clarity of expression it is quite ambiguous in meaning.

James's theory of emotion is embodied in the following oft-quoted sentence: "Bodily changes follow directly upon the perception of the exciting fact . . . and our feeling of the changes as they occur is the emotion." [1] The novel point in the theory is that the common sense account of the order in which the phenomena constituting emotion occur is inverted. The sequence is not, "We see a bear, are frightened and run," "We are insulted by a friend and strike"; but, "We see a bear, we run and are frightened," "We are angry because we strike," etc. This statement of the case appears on first approach clear enough. Let us see if it really is.

Consider the phrase 'exciting fact.' What does James precisely mean by it? Does he mean the mere sensory perception of the exciting object? Is it the visual perception of the bear that evokes the bodily changes, or is it the fact that the bear is interpreted as an object of danger? Those who have accepted the former meaning have thought to refute James by pointing out that we may sometimes see a bear (as at the zoo) and not run. To the experienced hunter the sight of a bear may not only not provoke the act of running away, but, on the contrary, the act of running toward the animal. By assuming the other interpretation of the phrase, one may avoid these objections, but if perception of fact includes the idea that the object perceived is also *interpreted* as an object of danger, at least part of being afraid precedes the act of running away, and James's theory loses much of its impressiveness.

We are not interested as to which of the two meanings was intended by James. We wish merely to call attention to the fact that the description of the first step in emotion as an exciting fact is an ambiguous statement and that there is definite need of distinguishing the *perception* of an object as a visual (or other sensory) presentation from the *interpretation* of it as a significant or threatening fact.

Consider now the phrase, 'feeling of bodily changes.' When one runs at the sight of a bear, does James mean that it is the kinesthetic sensations from the legs, resulting from their

[1] James, W., 'Principles of Psychology,' Vol. II, p. 449.

flexion and extension, that give rise to the feeling of fear? Or the sensations accompanying the filling of the tear ducts that give rise to the feelings characteristic of sorrow? Or is it the palpitation of the heart or the flushing of the cheeks or the increased peristalsis of the intestines, etc., that are the sources of these feelings? James never stated precisely which of these he intended to be included, although he seems to have had in mind primarily those sensations which are provoked by the vasomotor and respiratory changes. Those who have adopted his theory have usually added some other organic sensations. In general, the sponsors of the James-Lange theories insist that it is the group of sensations derived from the organs innervated by the sympathetic nervous system that give rise to the feelings experienced in emotion. Those opposed to this view have pointed out that there is no logical necessity for assuming that the sensations derived from stimulation of the smooth musculature enjoy this privileged position in the scheme of bodily changes that are observed to accompany emotional responses. Whatever side one espouses, however, it is necessary to recognize that we cannot, as James did, speak of bodily changes, pure and simple. There are many bodily changes which accompany emotion, some of them contributing to the subjective affective states called feelings, and others not. A distinction between the two must be made. Much of the discussion of the James-Lange theory has been more or less futile because this has not been done.

The above remarks are sufficient to illustrate the ambiguity which characterizes the current descriptions of what constitutes an emotion and to indicate the need of revising it. Attempts to do this have been made from time to time, but apparently without any positive result. It is therefore with some temerity that I approach the task. I appreciate, of course, that the term emotion having an accepted meaning in common language and, in addition, historical connotations both in philosophy and psychology, it cannot at this late date be rejected or given a wholly new signification. But while one may not appropriate the term, may not one suggest its elimination from rigorous scientific discussion, or at least de-

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mand that it be used only in its very general sense? I have attempted to do this in the following discussion; and proceeding upon the theory that one ought to adopt some specific label, however arbitrary, to distinguish clearly the various aspects of a given phenomenon, have further ventured to introduce some new terms.

The first obvious fact about any emotion is that it is initiated by some stimulus. The nature of the stimulus does not itself determine whether an emotion will be evoked. This will depend upon the particular significance which the stimulus may have for the individual experiencing it, and upon the conditions under which it occurs. Thus, the discharge of a gun is an upsetting stimulus for the man in the street, but one that may pass entirely unnoticed by the soldier in the trenches. The stimulus may be subjective or objective, moral or physical; it may be a sensation, an image, an idea, etc. A burn, the sight of a beautiful picture, the thwarting of a plan may equally be the source of an emotion. The question is *when* will a stimulus give rise to an emotion?

A stimulus will give rise to an emotion when it produces what I shall call *choc*. By *choc* I mean a general excitation of a large number of cranial and sympathetic nerve centers not habitually affected by the stimulus in question, and as a result of which there is a sudden alteration in the rate of physiological activity of the particular organs controlled by these centers. Any sensation, image, percept, or association pattern producing choc is an *emotive*. Whether a stimulus will serve as an emotive will depend upon the individual experiencing it and the particular circumstances in which it is being experienced. The same stimulus may not act as an *emotive* for different subjects, or for the same subject on different occasions. In general, we may conclude that a stimulus will act as an emotive when, because of its intensity or because of the particular significance it may have for the individual experiencing it, it is capable of producing choc.

In addition to giving rise to variations of physiological activity in individual organs or tissues, the effect of an emotive is further to provoke a specific orientation of the body as a

whole with respect to the exciting stimulus. At the sight of a bear, for instance, not only is there a likelihood for the heart to beat faster, for the knees to become shaky, etc., but here is also a definite impulse to move away from the bear, that is, to run. The reaction of running away, however, seems to differ from that of the palpitation of the heart in that it is a less invariable response to the situation. For instance, a seasoned hunter, who may have learned from experience that it is sometimes wiser upon being discovered by a bear to feign immobility, will, instead of running away, remain as motionless as possible although his heart will continue to palpitate in spite of him. In general, it appears that the physiological reactions of the individual organs are more or less invariable, whereas the bodily orientations are capable of modification and often are modified as a result of training, experience, or social custom. The rule is not without exception in either direction. Nevertheless, I believe it very important as well as useful to distinguish between the two types of objective response, and I suggest the term *physioaffective response* to designate the organic physiological reactions evoked by an emotive, and *affective behavior reaction* to designate the bodily orientation. We might reserve the current term, *expression of the emotions*, to cover both types of response. Thus, in the case of fear, variations of blood pressure and increased sweating are physioaffective responses, while flight from the source of danger is an affective behavior reaction. The increase in the rate of the heart and the modification of respiration in anger are physioaffective responses whereas the pugnacious attitude, the threatening voice, etc., are affective behavior reactions. Again, trembling upon hearing bad news would be a physioaffective response, wringing one's hands and shouting affective behavior reactions; crying might be either. All of them together could be included under the term bodily expressions of the emotions.

Continuing our analysis, the effect of choc through its direct or indirect action upon the brain is to give rise to a particular psychic state or *affect*. The quality or qualities which characterize this psychic state is what I should call *affective tone*. Conscious affect is feeling.

All affective tone is not identical, but seems to be qualitatively different in different 'emotions.' It is the perception of these differences in affective tone which enables us to distinguish subjectively the various 'emotions' from each other, as fear from anger, etc. What factors enter to determine the exact nature of the affective tone evoked, I am as yet not ready to state. Probably among them will have to be included the intensity of the *choc*, the mental make-up of the individual experiencing it, the character of the immediately preceding mental state upon which it was superimposed. And if we assume that there is a physiological basis for all affective tone, that is, a dependence upon the perception of certain 'bodily' changes, it will also depend upon the particular organs or muscles or tissues whose functioning has been modified.[1]

Summarizing our analysis, there are several aspects of it which deserve special attention. The first is the differentiation of the so-called 'exciting fact' into stimulus and *emotive*. The second is the introduction and elaboration of the concept of *choc*. The third is the differentiation of the expression-of-emotion, or bodily changes, into *physioaffective response* and *affective behavior reaction*. These distinctions are merely matters of objective analysis. In themselves they do not imply any particular theory of emotion. Their virtue lies in the fact that they eliminate that baneful ambiguity which has been the source of so much fruitless discussion and perhaps, also, investigation in the field of emotion.

[1] It may be of interest to point out that Allport, in his recent book which has attracted considerable attention through its discussion of the nature of emotion, has made this last factor the sole basis for the qualitative differentiation between the various emotions. He has in fact restricted the organs and tissues involved to include only the striate muscles, tendons, and joints. In his own words, "We propose that the differentiating factor arises from the stimulation of the proprioceptors in the muscles, joints, and tendons in the somatic part of the organism. . . ." (Allport, F. H., 'Social Psychology,' p. 91.) I make this citation to call further attention to the tendency toward over-simplification characteristic of current theory as well as description of emotion.

DISCUSSION

NEGATIVE TYPE REACTION-TIME SYMPTOMS OF DECEPTION

Issue

In February 1920, the writer published in the JOURNAL OF EXPERIMENTAL PSYCHOLOGY the results of certain experiments performed in the Harvard Laboratory, under the title 'Reaction-Time Symptoms of Deception.' The outstanding result reported was the discovery of a type of deception subject whose reaction-times during deception were shorter than while telling the truth. The writer termed this the 'negative type' of deceptive reaction-time symptoms.

In October 1923, Eva R. Goldstein published a report of a repetition of the writer's experiment under the title 'Reaction-Times and the Consciousness of Deception.'[1] Her results were practically identical with those found in the Harvard experiments, except that a greater proportion of subjects fell into the negative class; 7 out of 10 subjects giving negative type responses, and only 3 giving consistent positive type reactions. Miss Goldstein, however, came to the conclusion, based upon a study of her subjects' introspection, that shorter reaction-times during deception did not constitute 'a true deception type.' "The so-called negative type," she explains, (p. 573), "is not a type of response to deception, but a type of non-deceptive response to this particular experimental situation."

In support of this contention, she reported a second experiment with slightly changed conditions. Under these new experimental conditions, analysis of quantitative results disclosed "but one type of reaction-time behavior, . . . namely, a type which is characterized by increased reaction-times during deception," i.e. the positive type of deception response.

At first inspection of Miss Goldstein's article, it seemed to the writer that, since her results in repeating the Harvard experiments were identical with his own, the issue was wholly one of interpretation, which could, of course, be made with equal facility

[1] *Amer. J. Psychol.*, 1923, pp. 562–581. Report communicated from the Laboratory of the University of Pittsburgh by Jesse H. White and Gilbert J. Rich.

by all readers interested. While preparing subsequent results for publication, however, it has appeared to the writer that Miss Goldstein's alteration of the experimental conditions in her second experiment to produce, as she believed, increased 'consciousness of deception,' creates an issue with regard to the definition of deceptive attitude, or consciousness, which requires solution by experimenters in deception before presentation of further results.

The deceptive act required of each subject in the Harvard experiments was the performance of certain lists of simple arithmetical processes, in an order contrary to the one directed, and using on those lists an opposite arithmetical process to the one directed, with the purpose of deceiving the experimenter as to which lists were done according to direction, and which were not. Time was allowed before each list was started for the subject to do all the mental work required in reversing the experimenter's instructions; so that, once a deceptive list was begun, no more, or different mental processes were required of the subject than during the non-deceptive lists. Miss Goldstein altered this procedure by forcing the subject, when he chose to deceive the experimenter, to go through the additional mental act of reversing the arithmetical process to be used *at each process on the list*, including in her reaction-times for each deceptive list the time required thus to reverse nine separate items. It is not to be wondered at that all the lists on which such series of extra tasks were required consumed more time than the lists where less mental work was performed. The result reported, in short, might almost have been assumed as psychophysiologically axiomatic, without going to the trouble of conducting the experiment, except in so far as a quantitative measure of the handicap necessary to reverse the initial negative type reaction-time tendency was desired. It is, however, this clear demonstration of the predictable fact that deceptive reaction-times are substantially lengthened, in many instances of attempted deception, by additional mental work required to consummate the deception, that raises the issue: *Just what is 'consciousness of deception'?* What is properly to be included in that deceptive attitude which experimenters in deception tests are trying to find the symptoms of? Are we, after all, trying only to measure the bodily accompaniments of additional mental or physical labor which may be necessitated by deception? Or are we to define true 'deceptive attitude' (the term originally used by the writer [1]) as a characteristic motor setting and emotional condi-

[1] Marston, W. M., J. Exper. Psychol., 1917, 'Systolic Blood Pressure, Symptoms of Deception,' p. 162.

tion of the organism occurring during the act of attempting voluntarily to deceive another?

Argument

Previous to this time, all reaction-time writers seem to have been in agreement that increased reaction-times, or positive type responses during deception, were truly symptomatic of the subject's deceptive attitude.[1] The work of Miss Goldstein, when taken in conjunction with the writer's (as yet unpublished) quantitative studies of special aspects of deceptive reaction-times, seems to establish a grave doubt as to whether such is actually the fact. We find that all deception subjects became positive in type when the task of deceiving was made sufficiently difficult, whereas more than half of these same subjects proved negative in reaction-time type when the deceptive response was no more difficult to make than was the non-deceptive one. The conclusion seems to be suggested that the positive type of response is more strongly indicative of the difficulty of the deceptive task than of the attitude which may be obtained while the subject is preforming the task in question.

We have already noted Miss Goldstein's answer to this conclusion. She supposes that the shortening of reaction-times in the earlier experiment was not correlated with any consciousness of deception. She states that she bases this finding upon the published introspection of her negative type subjects. This introspection, since it closely corroborates that obtained in the Harvard experiments, may be accepted as typical of trained subjects' verbal reports under these experimental conditions. Subject B, 3 is quoted as saying, (p. 568): "I felt more confidence when disobeying and tried to gain speed from that fact." Evidently B, 3 succeeded. Yet this is not to be considered evidential of a true deceptive attitude, according to Miss Goldstein. Why not? Apparently because Miss Goldstein had made up her mind before beginning her experiment that the 'deceptive consciousness' she was looking for was composed of strain, excitement, and inhibitions. But surely no experimentalist can afford to predetermine his results. Nor can we assume, merely because previous experimenters have found strain and excitement present during many deceptive acts, that such strain and confusion constitute the true essence of the deceptive attitude.

Subject A, 1 is reported as introspecting, "I hurried on the

[1] Goldstein, E. R., *ibid.*, p. 580; also see bibliography, p. 581.

opposite cards, and went slower on the correct ones." A, 1's reaction-times accurately reflect this predetermined plan. Why may we not call this also a symptom of true deceptive attitude? Referring to all her negative type subjects, Miss Goldstein says: "The O's felt no strain and tension when disobeying, and did not experience a feeling of relief and relaxation when obeying. Nor did they feel any excitement when disobeying." Here, then, we find introspectively set forth still another aspect of a deceptive attitude which proved highly efficient in its results. All these characteristics of negative type deception-test introspection were originally summarized by the writer thus:[1] "No negative type subject reported 'excitement.' All, however, reported a definite 'plan.'"

It would seem, then, that Miss Goldstein and the writer are in perfect agreement: (1) That negative type deception subjects give evidence, both in their behavior and introspection, of an emotional attitude and motor setting excluding strain, tension, and excitement from levels of the nervous system correlated with introspective awareness, while accurately controlling the reactions of the entire organism according to a predetermined plan; and (2) that experimental conditions had been so simplified in the procedure reported as to exclude any possible influence of increased work of deception upon resulting reaction-times. In this negative type of reaction-time symptoms of deception therefore we find a certain definite state of the organism characteristic of a voluntary attempt to deceive another person; and we may be reasonably sure that this particular symptom is in no degree a measurement of additional mental or physical labor necessitated by the task of deceiving.

The ensuing situation is this: We have found a certain true deceptive attitude not attributable to any factors extrinsic to the organism's voluntary preparation for the attempt to deceive. But we have also found that, when the attempted task of deception becomes too difficult, this true deceptive attitude is lost, or overwhelmed in the total reaction-time behavior of the organism. When the majority of subjects (according to Miss Goldstein) can deceive as easily as not, they overdo the thing, and respond more rapidly than when they are not deceiving. They make evidently greater effort in deceiving than in acting honestly; and they make this greater effort according to a definite plan of action. But, when the examiner gets these same subjects into a corner, as it were, by

[1] Marston, W. M., J. Exper. Psychol., 1920, 'Reaction-Time Symptoms of Deception,' p. 82.

imposing conditions under which they cannot possibly react more quickly during deception, their plan breaks down, and (again according to Miss Goldstein) their excess of energy during deception becomes inhibited, in the form of "strain, tension, and excitement, and at times unpleasant feeling and lack of ease" (p. 576). The subjects, in short, who started out to deceive by using more energy and a special plan, are beaten by the experimenter, and are gradually forced to admit perceptions of defeat to nervous levels correlated with introspective awareness. The struggle, which started as a purely objective one, has been forced by their opponent inside the subjects' own nervous systems. They try to push hard but perceive the obstacle is not moving. This perception, in turn, inevitably inhibits the vigor of their push, with accompanying 'unpleasant feelings and lack of ease,' confusion, tension, and excitement. It is this latter state of conflicting motor settings within the organism that is evidenced, apparently, by increased, or positive type reaction-times; whereas the subject's own attitude, initially adopted for his attempt to deceive, was formerly evidenced by negative type reaction-time symptoms now overwhelmed. Shall we, then, include one or both of these settings of the organism in our attempted description of the 'true deceptive attitude'?

Experiment

If the negative type of reaction-time symptoms can persist throughout an actual instance of ordinary deception, we surely must include the motor setting which it evidences in any description of deceptive attitude. That it has so persisted in several association-reaction-time tests reported in the literature, was pointed out by the writer in a former paper.[1] During the fall of 1922, the writer performed an experiment at American University, Washington, D. C., further substantiating this conclusion.

Four students were sent from the classroom with sealed instructions, allotted by chance. Two sets of instructions directed 'innocent' actions. One instruction directed the finding and stealing of a check in another student's room, in the same building. And one instruction directed that a student return, by stealth, to a position advantageous for evesdropping, and listen to the instructor's remarks to the class describing the 'crime,' which was to be detected by combined association reaction-times and systolic blood pressure deception tests.

[1] *Ibid.*, p. 81.

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The usual association test was given, times taken with a stop-watch. The reaction-times of one student, on the crucial words, were clearly negative in type; and the crucial reaction-times of another student were slightly negative. These two students proved to be the ones who stole and listened, respectively. The reaction-times of the other two innocent students were both slightly positive on the crucial words. Whether this result was caused by chance emotional associations with the words in question, or was induced by suspicions that the words related to the 'crime' of which the subjects were being 'accused,' is not plain. All students were lawyers, and such a suspicion may well have occurred to them. Certain it is, however, that the experimenter and a majority of the class made correct judgments of the two guilty men, based upon their reaction-times alone; although the blood pressure test was required to determine which student stole and which one listened.

Conclusions

From the above analysis of available material, the writer would suggest the following tentative conclusions:

(1) Investigators of the deceptive attitude are not properly concerned with the measurement of psychophysiological accompaniments of additional mental or physical work necessitated by deception.

(2) The task before experimenters in deception is the description of conditions of the organism characteristic of the voluntary attempt to deceive.

(3) An emotional attitude resulting in increased energy being applied to the task of deception, with a motor setting rigidly adhering to a predetermined plan, characterize the deceptive attitude evidenced by negative type reaction-time symptoms.

(4) The initial deceptive attitude evidenced by negative type reaction-time symptoms may, in some cases at least, persist throughout an ordinary association reaction-time test without defeat; and must therefore, since it has been proved to exist from beginning to end of an act of voluntary deception, be accepted as one primary element in the description of true deceptive attitude.

(5) Positive type reaction-time symptoms in initially negative type subjects appear to be symptomatic of conflicts of impulses, and of inhibitions, following the subject's perception of the defeat of his initial deceptive attitude, rather than of the true deceptive attitude itself.

Whether the condition of the organism evidenced by the positive type of response must be included as an alternative element in our completed description of the deceptive attitude will depend, it would seem, upon whether positive type reactions can be proved to occur in any subject when there is present no perception, or imaginary picturing, of defeat of the deceptive undertaking. Experimentation directed toward the solution of that problem has not yet been attempted except in so far as the writer's reaction-time studies, as yet unpublished, may bear upon the issue.

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DISCUSSION

BLACK: A NON-LIGHT SENSATION

When a certain point of view is advanced we are reminded to notice very carefully the very good reasons offered in order that it be accepted. However, after careful deliberation, we often find that fundamentals are present, which, if they had been recognized, would help us reach entirely different conclusions. These fundamentals are the *real* reasons that must be taken into consideration. They are the elementary facts which must be followed in their course, lead they where they may.

At the outset we wish to present the position we take in regard to Black, and we hope, in the trend of our discussion, to prove the validity of our assumptions. Black (1) is a color, (2) is a non-light sensation attached to zero stimulation, (3) has only one degree of intensity. It is our desire that these points be considered and criticized in order to pave the way for a better understanding of the problem.[1]

There are six elementary colors: Red, Yellow, Green, Blue, White, and Black.[2] Four of these colors are chromatic: Red, Yellow, Green, and Blue. White and Black are achromatic. Five of these elementary colors, namely, the four chromatic sensations and White give rise to an intensity series. *Black has only one degree of intensity.*

Black *is a non-light sensation attached to zero stimulation.* Many agree to this, but in their eagerness to explain the occurrence of this black sensation they introduce a fallacy. For example, "Black, indeed, is wholly a contrast sensation; it has no physical stimulus; and you see deep black only in strong illumination." [3]

In strong illumination a black object reflects white light, with the result that we get *not* a deeper black but a darker gray. Zero stimulation with something added no longer gives us Black but Black plus another factor—in this case a light-sensation, namely, White—to produce a so-called black, which is a blend of Black and

[1] Neifeld, M. R., 'Ladd-Franklin Theory of Black Sensation,' PSYCHOL. REV., 1924, 31, 6.

[2] The elementary colors are always capitalized.

[3] Titchener, E. B., 'Beginners' Psychology,' p. 61.

White, commonly called gray. Illumination is not essential to get Black. We can get a perfect Black under two different conditions:

(1) By means of contrast (exhaustion of the light-sensation substance) produced by a surrounding White field.
(2) By means of the 'after-image' (exhaustion) produced by preëxposure of the retina to a bright light.

A non-light sensation is exactly what the term implies, zero stimulation. As soon as we introduce a new medium of stimulation we are not in a position to say that the sensation we get 'has no physical stimulus,' because it is known that illumination introduces a light-sensation stimulus by its reflection from the black to give a black-white or gray. To be informed first, that no physical stimulus is present and then to be told an active physical stimulus (light-sensation) is necessary to prove its presence deprives the value of the proof of its significance.

"Black is as much a sensation as white or any of the color hues; yet it is not due to stimulation of light waves at all." [1] No one can object to this statement because it covers the ground very accurately. In order to devise a correlate for Black the following is inserted, "though to get a distinct black sensation some nearby region must be stimulated by light." [2] We must also be aware of the fact that previous exposure to a bright light is an additional means of securing an absolute blackness.

All those intricate assumptions in an effort to make the Hering theory work become unnecessary as soon as it is assumed (what is the simple fact in the case) that:

(1) Blackness is due to zero stimulation.
(2) There is no possibility of varying the amount of Black. You can only change the *relative* amount in a black-white blend.

We cannot speak of different intensities of Black. The very substance, a light-sensation (White), which combines with it to form gray, cannot be used at the same time to give it degrees of intensity! The peculiar nature of the *B*—W series is that it is at once a quality series (like the blue-greens) and an intensity series (the white constituent varies in intensity as the illumination is made stronger and stronger).

Warren quoted by Fröbes, and others say that the Ladd-Franklin

[1] Warren, H. C., 'Elements of Human Psychology,' p. 82.
[2] *Ibid.*

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theory gives an explanation of all the phenomena of sight except the sensation black. The theory not only explains that phase, but also makes plain *why we have* in vision a sensation (Black) attached to zero stimulation; which occurs, of course, in no other sense region. I quote the passage:

> "Black may well be the sensation attached to the resting-stage of the cortical visual process. A state of non-excitation from the external world does not, in the case of the other senses, need to enter consciousness; but the visual sense of the spatial attribute is of extreme consequence, and if we were unconscious of objects which send to us no light there would be *lacunae* in the retinal optical field which would be most disturbing. This state actually occurs in the case of localized lesions in the cortical visual field; the ophthalmologists distinguish between *negative* and *positive* scotoma—in the latter, (disease of the retina or optic fibers) a black spot is seen in place of a position of the external field; in the former, nothing is seen. This fact alone goes far to prove that black is purely a cortical excitation which enters consciousness as a *mark* of the absence of retinal excitation." [1]

As we mentioned above, Black is a sensation of zero stimulation, and therefore cannot be a light-sensation, but *is a non-light sensation.* Since there can be only one value for zero, we get *one degree* of *subjective* intensity, and the series of grays is a result of the change in the subjective intensity of the White (light-sensation) constituents. The proof for this is not difficult. Take a blue-green with certain proportions of blueness and greenness, and it is possible to see them in different degrees of intensity. When we try to give a gray greater illumination we change not only the brightness but the quality by introducing more White (light-sensation).[2] For example, place a black piece of velvet in a black box with an aperture at one end. By enlarging or narrowing the slit we get different intensities which will be not those of Black (a non-light sensation) but Black plus White (a light-sensation). The result is a black-white or gray; zero stimulation is no longer present by itself! From this we see that there is one degree of intensity possible for Black and more than one, which our colleagues are wont to recognize, is simply a change in the White constituents. Black, then, *is a color produced by zero stimulation correlated with a non-light sensation which gives one degree of intensity.*

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[1] Ladd-Franklin, C., 'Vision,' *Dictionary of Philosophy and Psychology*, 1902.

[2] *Ibid.*, 'Tetrachromatic Vision and the Genetic Theory of Color,' *Amer. J. Physiol. Optics*, 1923, 4, 403–415.